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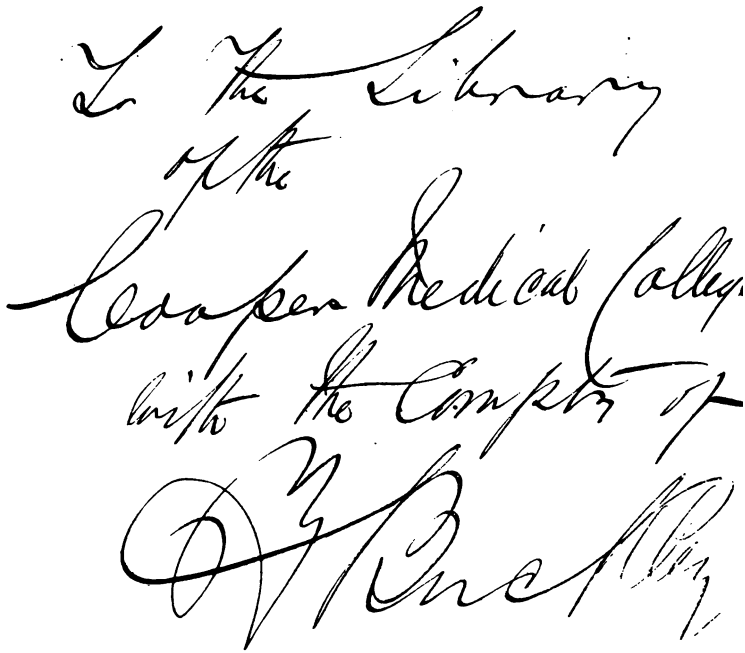

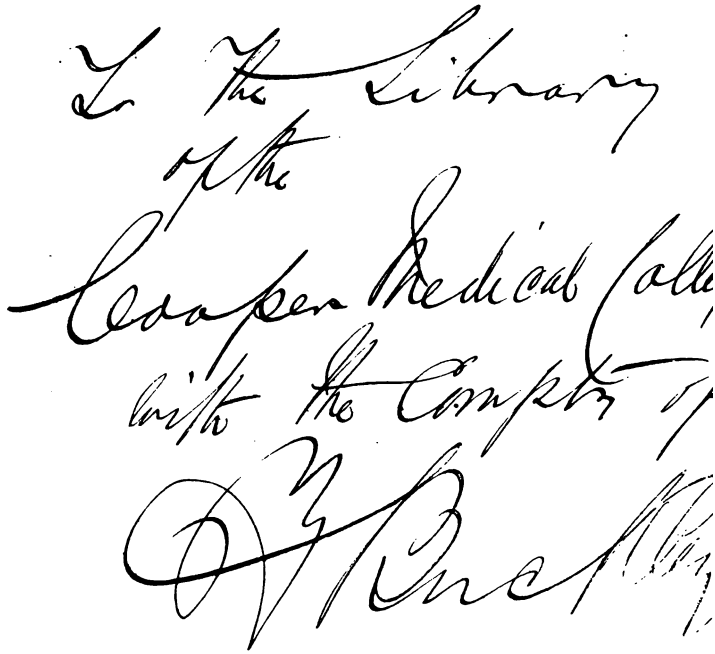

CEREBRAL HYPERÆMIA;

DOES IT EXIST?

A CONSIDERATION OF SOME VIEWS OF

DR. WILLIAM A. HAMMOND,

BY

C. F. BUCKLEY, B.A., M.D.,

FORMERLY

SUPERINTENDENT OF HAYDOCK LODGE ASYLUM, ENGLAND.

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CEREBRAL HYPERÆMIA.

DOES IT EXIST?

POSSIBLY among the developments of that most civilizing and beneficent of inventions—the printing press—there is none more curious, and certainly none more detrimental to public morals, than the ever-increasing plague of medical literature, so to speak, with which we are everywhere environed. But apart altogether from those infamies that are laid at our door-steps, or obtruded into our hands at the street corners, the medical practitioner has a grievance of his own of another order. "To keep up with the times" he must perforce wade daily through no end of meaningless magazine literature, and must necessarily pour over *seriatim* those ephemeral "works" which in nine cases out of ten appear to be published for no other purpose than to herald abroad the "genius" of some particular individual. There seems to be no conceivable form of extravagance that has not got its specious advocate; and

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for what they may be worth, trusting that whatever defects or errors shall have a place in these pages may not be overlooked by abler pens, but dealt with in the same spirit—the spirit of truth-seeking—which has been the only incentive to their utterance here. In this way only has real progress been made in science generally, as well as in medicine: and it is my individual opinion that if a stricter censorship of medical works could be more generally adopted, we should all be saved much valuable time now devoted to re-reading over and over the attenuated repetitions of our standard authors, or the sorry verbiage of semi-educated novices.

"*Non ego ventosæ plebis suffragia venor*
Impensis cœnarum et tritæ munere vestis."

CHAPTER I.

INTRODUCTORY.

To the careful reader of this Monograph on "Cerebral Hyperæmia," one of the earliest impressions, it seems to me, must be the strange medley of positive contradictions contained within its own one hundred and odd pages; and whether he contrasts one chapter with another, or even compares different portions of the same chapter, the result will remain the same.

Though desirous above all things to endeavor to do justice to the work as a whole, and avoid all semblance of partial or hyper-criticism, it can hardly be out of place to call attention, at the outset, to such glaring inconsistencies as those wherein the author at one moment announces to us the novel doctrine that chlorosis is the result of "cerebral hyperæmia," and immediately after informs us that this same "cerebral hyperæmia" has, for its leading symptom, "the *redness of the face and throbbing of the cephalic arteries, indicating an increased supply of blood to the brain.*"

Again, as proof positive—nothing less—of the existence of the malady, the deleterious effects of "*strychnia* and *quinine among other things which we know to increase the amount of blood in the brain,*" are pointed out in no doubtful language, and, very few pages later, we find recommended:

"*Strychnia Sulph.* gr. i: *Quin. Sulph.* ʒi: *Ferri Bromid.* ʒss: Acid *Hydrobrom.* ℥x: *Glycerini* ℥ii. Dose, a teaspoonful in water three times a day after meals."

Another very remarkable peculiarity of the work is that while it claims to deal with a hitherto unrecognized undiscovered condition demanding a distinctive name in our nosology, there is not a single pathological fact recorded in its pages to uphold the position assumed: and it is not too much to say that the chapter devoted to "Morbid Anatomy" is one of the boldest paradoxes in medical literature, inasmuch as *all* the "cases" of the *new disease* recorded in the monograph—*all recovered!* Hitherto in medical literature the rule has been that, in order to establish the identity of a previously unrecognized disease, which was to be henceforth designated by a distinctive nomenclature, some new phase of morbid activity has been first demonstrated. But Dr. Hammond reverses this rule, gives us a chapter on "Morbid Anatomy" without a line on morbid anatomy in the chapter, but instead relates

a number of "cases" which may safely be referred to other and more generally recognized causes, and which "cases" are of necessity bereft of all "morbid anatomical" interest for the simple plain reason that not one of them was ever subjected to post-mortem examination. Not unlike statistics in the hands of the adroit politician, "cases" in the hands of the medical advocate are brought forward to sustain every conceivable theory.

By way of illustrating this remark, I may be permitted to digress briefly from the subject immediately under notice, and take a "case" from the larger work of Dr. Hammond, "Diseases of the Nervous System." It is the first case in the chapter on "Cerebral Softening," p. 161, and though incurring the risk of becoming tedious it is necessary to quote the author's remarks at length in order that the reader may judge for himself.

"Mr. R., a gentleman twenty-four years of age, awoke one morning, about the middle of March, 1870, with a sensation of numbness extending through the whole of the left arm and leg, and with a feeling of vertigo which was insupportable when he arose from the bed. He sat down in a chair, and while in this position was conscious of a buzzing sound in the right ear. In the course of half an hour the vertigo passed off, but the numbness and sound in the ear remained, and he occasionally saw

double. In a few days afterward he noticed a slight difficulty of articulation, owing to apparent thickness of the tongue, and about the same time observed that in the morning the pillow was wet with the saliva which had run from his mouth during sleep. His uncle, a wealthy gentleman of this city, sent him off traveling, but he returned in a few weeks with *loss of power in the left arm and leg*,* which had begun to be manifested to a slight extent before his departure. He came under my charge May 15th, 1870.

"At this time the paralysis of both motion and sensation was well marked on the left side, as shown by the æsthesiometer and dynamometer. The line made by the dynamograph with the right hand was perfectly straight, while that made by the left was at an angle of forty-five degrees with the other. In his conversation he clipped his words, and he sometimes left out the smaller ones. His memory he stated was materially impaired. There was almost constant headache over the whole frontal region, and attacks of vertigo were frequent. There was no marked paralysis of the face, though the muscles of both sides were paretic, and he frequently had double vision. The *right pupil*

* I would draw early attention to the completeness of the hemiplegia in the case.

writer are not always entitled to the credit commonly accorded them hitherto, but, on the contrary, are to be received with judicious caution.

Taking the report as it stands, and especially those portions of it relating to the complete hemiplegia and the quick and complete recovery, it does seem, to say the least of it, a very unique case of cerebral softening; and this view of its singularity is confirmed by Dr. Hammond himself, in his treatise on Cerebral Hyperæmia, as will be seen later on. But in order to form a correct judgment on the matter it is necessary to consult the best opinions among our standard authorities, always bearing in mind that the writer himself in this case attributes the cerebral softening to "*disease of the capillaries.*"

Let us first refer to Hughes Bennett, whose painstaking clinical labors are surpassed by few, if any, of the writers of this century. Under the heading "Cerebral Softening," p. 354, American edition 1870, he says:—

"From a careful analysis of numerous cases of cerebral softenings I have arrived at the conclusion that they may originate in six ways: 1st, from exudation which is infiltrated among the elementary nervous structures; 2d, from a mechanical breaking up of these structures by hemorrhagic extravasations, whether in large masses, or infiltrated in small isolated points; 3d, from fatty degeneration of

the nerve cells independent of exudation; 4th, from the mere inhibition of serum, which loosens the connection between nerve tubes and cells; 5th, from mechanical violence in exposing the nerve centres; and 6th, from putrefaction."

Further on, at page 355, in treating of hemorrhagic softening, he remarks: "There are none of the granule cells so characteristic of an inflammatory softening, although they may appear later as the result of exudation from the cerebrum surrounding the clot." This is worthy of note as showing that he desires not to confound hemorrhagic with inflammatory softening —a fact which the text more fully elucidates. Desiring not to cumber the memory with extended quotations, I beg to refer the reader now to p. 357. Herein the author remarks: "From a careful analysis of 32 cases of softening of the nervous centres, which I published in 1842–43, it was shown that different symptoms were connected with exudative or inflammatory, from those which occurred in non-inflammatory softening. In 24 of these cases in which cerebral softening was observed, granular corpuscles were present in 18, whilst in 6 no trace of these bodies could be found. On analyzing the symptoms of the 24 cases a marked difference was found between those resulting from the two lesions. Thus in the cases where *only* inflammatory softening was present, well marked symptoms invariably existed,

was largely dilated and insensible to light. Ophthalmoscopic examination showed the left eye to be perfectly normal, but the retinal vessels of the right were smaller and straight, and the choroid was paler than natural. Upon inquiry I ascertained that he had given extraordinary attention to his business for a period of several months before the attack of numbness, frequently being up making calculations till three o'clock in the morning, and thus depriving himself of the necessary amount of sleep.

"My opinion was that he was suffering from incipient softening of the brain due to disease of the capillaries, which in its turn resulted from cerebral congestion and exhaustion. I was further of the opinion that the lesion involved the right hemisphere and motor tract. I prescribed the phosphide of zinc in the dose of the tenth of a grain, with half a grain of nux vomica in pill three times a day, with the constant galvanic current three times a week—the latter to be derived from fifteen of Smee's cells, and to be passed from forehead to occiput for three or four minutes at a time. *At the end of ten days* he had lost his diploplia; the pupil of the right eye had regained its natural diameter and irritability, and the vertigo and headache had notably diminished. The treatment was continued, and at the end of a month he had recovered the sensibility and power on the paralyzed side to such an

extent, and had improved so much in other respects, that I advised him to take a short journey. He was absent two weeks, during which period he continued to take the pills as before, and on his return was to all appearance well. He has since remained in excellent health."

This case is of uncommon importance from more than one standpoint, and the reader can only do it justice by a careful consideration of every line.

First of all, if the diagnosis be correct, it illustrates the marvellous, we might indeed say magical effects of phosphide of zinc, nux vomica, and the constant galvanic current in curing diseases commonly considered hopeless as regards complete and permanent recovery; and all this, too, in the brief short span of TEN DAYS.

Secondly, it is no doubt intended as the type of this condition under consideration, viz., Ramollissement or Brain-Softening; and so given the place of honor in the record of cases: although its pathology is rather vague, inasmuch as we are told that the softening was due "to *disease* of the *capillaries*, which in its turn resulted from cerebral *congestion* and *exhaustion*"—whatever this may mean.

And lastly, it is important, because if the evidence of brain-softening herein is defective; nay, if the weight of opinion be entirely opposed to any such assumption, it follows that the observations of the

writer are not always entitled to the credit commonly accorded them hitherto, but, on the contrary, are to be received with judicious caution.

Taking the report as it stands, and especially those portions of it relating to the complete hemiplegia and the quick and complete recovery, it does seem, to say the least of it, a very unique case of cerebral softening; and this view of its singularity is confirmed by Dr. Hammond himself, in his treatise on Cerebral Hyperæmia, as will be seen later on. But in order to form a correct judgment on the matter it is necessary to consult the best opinions among our standard authorities, always bearing in mind that the writer himself in this case attributes the cerebral softening to "*disease of the capillaries.*"

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such as loss of consciousness, *preceded* or *followed* by dulness of intellect, contraction and rigidity of the extremities, or paralysis. On the other hand, in the six cases of *non-inflammatory* softening there *was no paralysis* or *contraction*, and no *dulness* or *disturbance* of the *intellect*. Again, in the four cases where both lesions were present, symptoms *were always* observed in the side of the body *opposite* to *the seat of the* inflammatory softening, but *none existed in the opposite* side *in the non*-inflammatory." It will be remembered that Dr. Hammond's supposed typical case, which he plainly advances as one of the non-inflammatory nature, had paralysis of both motion and sensation on the left side, and that he diagnosed softening from disease of the capillaries in the motor tract of the right side; thus, by mere sweeping surmise directly opposing the careful analysis of Hughes Bennett.

Again, Trousseau, who certainly is not at his best in his chapter on this subject, inasmuch as he seemed too much inclined to adhere closely to the tenets of Racamier without personal investigation, makes the following remarks (p. 63, vol. 2, Paris Ed. 1868):

"Mais lorsque l'intelligence est troublée, sans être tout a fait abolie, lorsque la sensibilité est obtuse sans être complètement eteinte, en même temps qu'il y a *paralysé complète du movement*, comme cela existe chez notre malade de la salle Sainte Agnes,

on doit toujours suivant Racamier, croire qu'il y a hemorrhagie liée au ramollissement ou bien qu'on a affaire à cette forme d'hemorrhægie qu'on a appellée hæmorrhægie capillaire, laquelle se fait ordinairement dans une portion ramollie de l'encephale, hæmorragie capillaire caracterisée a lautopsie par la présence soit d'une multitude de petits foyer hemorrhægiques, restant parfaitement isoles les un des autres, ou se réunissant de façon a former des foyers plus ou moins grand. Dans ces cas, mais dans ces cas seulement, le célèbre médecin de l'Hotel Dieu était tenté d'admettre le ramollissement préalable que Rochoux regarde comme la condition organique, le travail pathologique précurseur obligé de toute hemorrhagie cérébrale." Thus the distinguished teacher informs us that impaired intelligence and blunted sensation power coupled with *complete* hemiplegia are more or less certain indication of the existence of hemorrhage, either as a cause or consequence of ramollissement. Among other facts observable from Trousseau's brief remarks on this subject, it is perhaps worthy of note, that the cases of softening he alludes to, are all paralysed on the *right side*, and invariably *terminate* fatally.

Let us now turn to Russel Reynolds' work (vol. 2, p. 472, London Ed. 1868).

"From *hemorrhagic apoplexy acute softening* may be sometimes distinguished by the following con-

siderations: In hemorrhage there is often some evidence of either congestion or of shock; in softening, there may be an entire absence of both. In hemorrhage the attack frequently *occurs at night:* the patient goes to bed apparently well, and wakes in the morning feeling as usual, but on attempting to move finds that one side is paralyzed. In hemorrhagic apoplexy the attack is often absolutely instantaneous, in softening it is gradual. In the former there may be *not* the *least*—even momentary—*confusion of mind*, whereas in the latter there is *distinct mental perturbation* and insufficiency. In hemorrhage when the intellect is profoundly affected, as it sometimes is at the outset of the attack, there is often a *rapid restoration*, and in the course of a quarter of an hour the patient's mind is as clear as it was before: in softening *per se* there is less distinct mental obscuration at the *commencement, and little or no subsequent recovery.* In hemorrhage there is sometimes alarm, and not unfrequently anxiety and depression, whereas in softening there is commonly too much confusion of thought for any definite apprehension to be entertained, and sometimes there is transient excitement or mild delirium.

"Sensibility is often unaffected in hemorrhage, it rarely escapes altogether in an attack of softening. In the former, there is, as a rule, *unilateral anæsthesia, which rapidly diminishes or disappears;* in

the *latter* there is *dulness of sensation*, with morbid feelings of coldness, numbness, tingling, &c., which persist.

"The paralysis in hemorrhage is *typically hemiplegic;* in softening it is *more irregular*, and more *closely limited.* In the former there is neither rigidity nor convulsions, unless the coma be profound, and the paralysis extensive; in the latter there is *often* either *twitching* or *tonic spasm* even when the paralysis is slight and the mental perturbation trivial.

"In hemorrhage there is frequently hypertrophy of the heart with granular degeneration of the kidneys: in softening there is more commonly a weakened heart with *valvular disease.* Hemorrhage may occur in a person of strong limb and general good health, whereas *softening* is more *common in the aged*, the enfeebled, and those who have suffered from exhausting diseases,* or still more exhausting causes."

I would now refer to the work of Niemeyer (vol. 2, p. 181). Translation by Drs. Hackley and Humphreys, New York, 1877. It is impossible to convey a correct idea of the views of the eminent author without more extended quotations than the character of this essay will admit of, but a careful

* By bearing well in mind the salient points of this article, the reader will, as I believe, rarely err in his differential diagnosis between softening and hemorrhagic apoplexy.

study of his articles on "Partial Anæmia" and "Cerebral Apoplexy" will convince the reader that in a case such as the "typical" one reported above, Niemeyer's opinion would materially differ from that expressed by Dr. Hammond. Thus, at page 101, he says;

"Since thrombosis of the cerebral vessels most frequently depends ultimately on atheromatous degeneration of the vascular walls, and as this occurs chiefly *in old age*, we are more apt to suspect thrombosis and *consequent softening of the brain* in an old decrepit person, who has the symptoms of severe brain trouble, than in a young vigorous one having the same symptoms. If the peripheral arteries be rigid and tortuous, there is still greater presumption that the arteries of the brain are also degenerated, and that the brain symptoms are due to this degeneration. However, the condition of the peripheral arteries does not furnish any certain proof of that of the cerebral arteries. In many cases the degeneration is confined to the latter; in other rarer cases, while the peripheral arteries are extensively degenerated, the cerebral arteries remain free. Moreover, partial anæmia of the brain, and softening of the brain due to necrosis of the anæmic portion, is not the only brain disease caused by atheromatous degeneration of the cerebral arteries. Experience shows that atheromatous degeneration of the walls

of the vessels usually induces dilatation of the larger arterial trunks, and on the contrary *contraction* of the *smaller arteries.* The atheromatous arteries of the brain are also usually contracted for a long time before they are closed by thrombosis. Hence the symptoms of thrombosis, or rather of the partial anæmia and the partial necrosis of the brain depending on it, are almost always preceded by premonitory symptoms, either of disturbance of circulation, induced by the contraction of certain cerebral vessels, or by symptoms of senile cerebral atrophy, which has been caused and hastened by degeneration of the cerebral vessels. The patients complain of pain in the head, dizziness, ringing in the ears, flashes before the eyes, loss of memory and power of thought; they are apathetic and indifferent, and much inclined to sleep, but their sleep is much disturbed by uneasy dreams. As a rule, very small arteries are at first obstructed, either by the atheromatous process causing their entire obliteration, or because their calibre is contracted, and then obstructed by a thrombosis. The anæmia resulting from closure of these small vessels is limited in extent, and hence may readily be removed by an increased supply of blood through neighboring vessels. We must bear this in mind, when in a marasmic patient, who for some time has had the brain symptoms, that are designated as premonitory, lim-

ited regional symptoms (head symptoms) occur, and, after lasting for a time, disappear again. Among these symptoms are *inability to say different words*, loss of memory for certain names and numbers, pain or a feeling of formication or of certain limbs going to sleep, occasionally only of certain fingers or toes, contractions or paralysis, which are also occasionally limited to certain fingers or toes, etc.

"Many authorities have explained this variation of symptoms, particularly the appearance and disappearance of paralysis, as a peculiar symptom of softening of the brain. *This is a false view of the matter.* In cases where the symptoms have presented this variation, if softening of the brain be found on autopsy, the softening did not occur at *the time the symptoms changed, but at a later period*, when they were constant. On the other hand, the occurrence and disappearance of circumscribed paralysis are certainly to some extent characteristic of partial anæmia of the brain resulting from atheromatous degeneration and thrombosis of small cerebral arteries, and are *rapidly* removed again, by the establishment of a collateral circulation. (The occurrence and disappearance of circumscribed paralysis do not render it certain that there is thrombosis of the small arteries of the brain; the *same symptom is also seen* from small extravasation.)"

Again, in the chapter on cerebral apoplexy, the author makes this significant statement which few pathologists will attempt to gainsay: "A large apoplectic clot, destroying the corpus striatum or thalamus, leaves a *hemiplegia* that never disappears; only small clots in these parts by which the filaments and ganglions are not broken down, but only *pressed apart, leave paralysis which is occasionally temporary.*"

The reader will, no doubt, observe that though there be marked shades of difference in the opinions of the authors here quoted, yet there is sufficient concurrence of testimony to convince ordinary minds that, in the absence of *post-mortem* verification, Dr. Hammond's "typical" case has more of the characteristics of cerebral hemorrhage (probably of the capillary kind) than of brain softening. Among the points specially noticeable are—the age of the patient; the mode of inception; the absence of cardiac disease; the well-defined hemiplegia; the absence of any defined intellectual disturbance; and the quick and complete recovery. If I may be permitted to add my personal testimony to that of the distinguished authors above quoted, I can only say, that according to observations which were at one time not inconsiderable, intellectual disturbance, as shown either in the memory or emotions, and a persistent defect of muscular management (so to

speak) in some special direction—not hemiplegia, however—were more or less marked in all the cases of brain softening that were verified as such on *post-mortem* examination.

To return now to the work more immediately under notice, viz., "Cerebral Hyperæmia," we find in the Introduction, p. 15, the following: "Thus, several years ago, desiring to ascertain the effects of excessive mental labor upon the brain, as indicated by the excretion of urine, I performed a series of experiments on myself, by which it was clearly ascertained that the solid matter eliminated by the kidneys was notably increased in direct relation with the extent to which the brain was worked. All this was for the time being at least within the limits of health, but by persevering with the experiments, and carrying the mental exertion to a still higher point, a stage would have been reached at *which the decomposition of brain substance would have been greater than the formation processes, and then disease would have existed.* I would have been living, as it were, on my brain capital instead of the income, and brain bankruptcy would have been only a question of time, just as it is in financial matters."

This, no doubt, is a very instructive and elegant piece of medical literature, and the simile may be perfect; but in order to satisfy the curiosity of the medical reader, for whose enlightenment, possibly,

it may have been published, and on whom the financial metaphor may be completely lost, it would be only a simple act of justice to give some details of the experiments leading up to this terrible brain bankruptcy, inasmuch as most of the physiologists whom we are taught to regard as authority, claim that the quantity and quality of the aliment received into the stomach has a direct and positive influence on the amount of solid matter eliminated through the kidneys—that, in fact, highly-fed dogs can pass, in proportion to their body weight, more solids from their kidneys than very learned and laborious professors and physicians.*

In relation to this subject, it may be well to compare Foster's Physiology, p. 462, Ed. 1880: "When a diet of lean meat, as free as possible from fat, is given to a dog, which has previously been deprived of food for some time, and whose body, therefore, is greatly deficient in flesh, it might be expected that the great mass of food would be at once stored up, and only a small quantity be worked off immediately, as an additional quantity of urea, occasioned by the increased labor thrown on the economy by the very presence of the food. *This, however, is not the case.* The larger portion passes off as urea at once, and only a comparatively small quantity is

* See experiments by Bichat and others.

retained. If the diet be continued, and we are supposing the meals given to be ample ones, the proportion of the nitrogen which is given off in the form of urea goes on increasing until at last a condition is established in which the nitrogen of the egesta exactly equals that of the ingesta." But, possibly, Dr. Hammond may refer to the phosphates, although whenever the solids of the urine are computed in the aggregate, urea must necessarily occupy first place in our attention, inasmuch as it constitutes nearly one-half of all the solids found in this excretion. Supposing, then, that phosphates—as the brain substance is specially phosphorized—and other salts, in the uncertainty that necessarily surrounds the author's experiments, had some place in his calculations, let us again compare Foster, p. 408:

"The phosphates are derived partly from the phosphates taken as such in the food, partly from the phosphorus or phosphates peculiarly associated with the proteids, and partly from the phosphorus of certain complex fats, such as lecithin. When urine becomes alkaline, the calcic and magnesic phosphates are precipitated, the sodium phosphates remaining in solution. The sulphates are derived partly from the sulphates taken as such in food, and partly from the sulphur of the proteids. The carbonates, when occurring in large quantity, generally

have their origin in the oxidation of such salts as citrates, tartrates, etc. The bases present *depend largely on the nature of the food taken.* Thus with a vegetable diet, the excess of the alkalies in the food reappears in the urine. With an animal diet, the earthy bases in a similar way come to the front."

Thus we see, as stated, that the chief solid constituents of the urinary secretion are directly dependent on the quantity and quality of the ingesta, *i. e.*, according to the most exhaustive and recent experiments hitherto published; and hence it would be only a slight concession to proper professional curiosity to inform us by what course adopted Dr. Hammond has been enabled to arrive at a totally different conclusion. There are, indeed, comparatively few ultimate facts hitherto ascertained in physiology, but when we learn from numerous experimenters and observers that the nervous system is the last to change*—in the physiological decay of starvation, for example—in conjunction with the facts above stated; surely such sweeping assertions as those made by Dr. Hammond, unsupported by any testimony save his own *ipse dixit*, cannot be very instructive to those seeking information on an intricate subject.

* It will be remembered that Dr. Hammond, during his experiments, was, as he tells us, within the physiological pale.

The following, also from Foster, p. 450, as coinciding with the commonplace observation of every practical physician cannot fail to be interesting in connection with this subject:

"Thus the loss during starvation fell most heavily on the fat, indeed nearly the whole of this disappeared. Next to this fat, the glandular organs, the tissues which we have seen to be eminently metabolic, suffered most. Then came the muscles, that is to say, the skeletal muscles, for the loss in the heart was very trifling. Obviously this organ, on account of its importance in carrying on the work of the economy, was spared as much as possible. It was, in fact, fed on the rest of the body." The same remark applies to the brain and spinal cord, "in order that life might be prolonged as much as possible, these important organs were nourished by materials drawn from less noble organs and tissues."

The reader may properly object—"But all these experiments differ essentially in their conditions from any that may be made during brain or intellectual labor!" Grant this. Yet do they not distinctly develop two facts? 1st. That the solid constituents of the urine depend largely, if not completely, on other circumstances than intellectual labor; and 2d. That in the ordinary physiological metabolism of the body—and surely ordinary brain exercise produces this only—the brain and spinal

cord are among the last, if not *the* last, portions of the economy to suffer observable change or loss, or "bankruptcy." Hence, to my mind, the assertion of Dr. Hammond, that perseverance in intellectual labor would have led to brain disease—and this merely from unrelated experiments on urine—has no weight whatever, and should even be discarded as positively vicious, in so far as it may deter many a timid reader from pursuing the course of study congenial to him. Of course it is possible to injure the brain, or destroy its usefulness, by any form of excessive or injudicious intellectual pursuit, such as we so often observe among the readers of meretricious works on medicine; but most untechnical observers, whose ideas of medicine are not restricted to chemical analysis of the urine, will admit that among the true brain-workers of the world, whose habits otherwise are of no enfeebling nature, there exists as little brain disease, as little brain "bankruptcy," as among any other class of the community. Indeed, taking merely a general review of the problem, it seems to be established that judicious brain labor, far from being an enfeebling factor in our human existence, is really a powerful invigorator, not merely of the brain proper, but of our whole physical constitution also, as witnessed by the great average longevity of the most laborious intellects of this and other countries.

Now, as to the general problem of cerebral hyperæmia, as far as the strict meaning of the term is concerned, I doubt very much if any intelligent physician will question its existence at the present stage of physiological and pathological development. The admitted anatomical conditions of the cerebral vessels, and the structures immediately surrounding them; analogy with other important organs throughout the body; clinical observations of constant occurrence; and daily *post-mortem* examinations of unimpeachable correctness, all point to this conclusion in unmistakable language. Every writer of note on cerebral diseases describes the existence of active and passive cerebral congestions, and Dr. Hammond, in his work on "Diseases of the Nervous System," dwells at some length on the same distinctions. But when the learned author adds another form of cerebral hyperæmia to those two, and distinct from them, I claim, with all due deference, that he not only has not proven its existence in anything hitherto published, but that he actually assumes a condition to exist which is impossible. At the threshold he tells us that this new *disease* "consists primarily of an increase in the quantity of blood circulating in the cerebral arterial vessels." How this condition can exist without a similar increase continuing into the venous circulation, thereby creating the active congestion uni-

versally recognized, is a problem that my comprehension cannot grasp. The idea is opposed to all our conceptions of the circulation of the blood, indeed to all principles of the laws governing fluids and their motions, and yet it is laid down here with as much nonchalance and dogmatic indifference to criticism as if it had been demonstrated beyond a question to our unaided vision. Let us again consult Foster * (p. 148):

"When fluid is driven by an intermittent force, as by a pump through a perfectly rigid tube (or system of tubes), at each stroke of the pump there escapes from the distal end of the system just as much fluid as enters it at the proximal end. The escape, moreover, takes place at the same time as the entrance, since the time taken up by the transmission of the *shock* is so small that it may be neglected. This result remains the same when any resistance to the flow is introduced into the system. The force of the pump remaining the same, the introduction of the resistance undoubtedly lessens the quantity issuing at the distal end of each stroke, but it does so simply by lessening the quantity entering at the proximal end. The income and outgo remain equal to each other, and occur almost at the

* I refer so frequently to this author, because, whilst giving an excellent summary of the most recent physiological labor, his work is singularly free from the advocacy of mere theories.

same time. And what is true of the two ends, is also true of any part of the course of the system, so far, at all events as the following proposition is concerned, that in a system of rigid tubes, either with or without an intercalated resistance the flow caused by an intermittent force is in every part of the tubes intermittent synchronously with that force.

"In a system of elastic tubes in which there is little resistance to the progress of the fluid, the flow caused by an intermittent force is also intermittent. The outgo being nearly as easy as the income, the elasticity of the walls of the tubes is scarcely at all called into play. These behave practically like rigid tubes. When, however, sufficient resistance is introduced into any part of the course, the fluid, being unable to pass by the resistance as rapidly as it enters the system from the pump, tends to accumulate on the proximal side of the resistance. This it is able to do by expanding the elastic walls of the tubes. At each stroke of the pump a certain quantity of fluid enters the system at the proximal end. Of this only a fraction can pass through the resistance during the stroke. At the moment when the stroke ceases, the rest still remains at the proximal side of the resistance, the elastic tubes having expanded to receive it. During the interval between this and the next stroke,

the distended elastic tubes, striving to return to their natural undistended condition, press on this extra quantity of fluid which they contain, and tend to drive it past the resistance. Thus in the rigid system (and in the elastic system without resistance) there issues from the distal end of the system, at each stroke, just as much fluid as enters at the proximal end, while between the strokes there is perfect quiet. In the elastic system with resistance, on the contrary, the quantity which passes the resistance is only a fraction of that which enters the system from the pump, the remainder, or a portion of the remainder, continuing to pass during the interval between the strokes. In the former case, the system is no fuller at the end of the stroke than at the beginning; in the latter there is an accumulation of fluid between the pump and the resistance, and a corresponding distension of that part of the system, at the close of each stroke—an accumulation and distension, however, which go on diminishing until the next stroke comes. The amount of fluid thus remaining after the stroke will depend on the amount of resistance in relation to the *force of the stroke*, and on the distensibility of the tubes; and the amount which passes the resistance before the next stroke will depend on the degree of elastic reaction of which the tubes are capable. Thus, if the resistance be very considerable in

relation to the force of the stroke, and the tubes very distensible, only a small portion of the fluid will pass the resistance, the greater part remaining lodged between the pump and the resistance. *If the elastic reaction be great, the large portion of this will be passed on through the resistance before the next stroke comes.* In other words, the greater the resistance (in relation to the force of the stroke) and the greater the elastic forces brought into play, the less intermittent, the more nearly continuous will be the flow on the far side of the resistance. If the first stroke be succeeded by a second stroke before its quantity of fluid has all passed by the resistance, there will be an additional accumulation of fluid on the near side of the resistance; an additional distension of the tubes; an additional strain on their elastic powers, and in consequence, the flow between the second stroke and the third will be even more marked, than that between the first and the second, though all three strokes were of the same force, the addition being due to the extra amount of elastic force called into play. In fact, it is evident that, if there be a sufficient store of elastic power to fall back upon, by continually repeating the strokes, a state of things will be at last arrived at, in which the elastic force, called into play by the continually increasing distension of the tubes on the near side of the resistance, will be sufficient to drive through the

resistance, in the interval between *each two strokes just as much fluid as enters the near end of the system at each stroke.* In other words, the elastic walls of the tubes will have connected the intermittent into a continuous flow. The flow on the fore side of the resistance is in this case not the direct result of the strokes of the pump. All the force of the pump is spent, first in getting up, and next in keeping up the over-distension of the tubes on the near side of the resistance; it is the over-distended tubes which are the cause of the continuous flow, by emptying themselves into the far side of the resistance, at such rate that they discharge through the resistance during a stroke, and in the succeeding interval, just as much as they receive from the pump by the stroke itself.

"This is exactly what takes places in the vascular system. The friction in the minute arteries and capillaries presents a considerable resistance to the flow of blood through them into the small veins. In consequence of this resistance the force of the heart's beat is spent in maintaining the whole of the arterial system in a state of over distension, as indicated by the arterial pressure. The over distended arterial system is, by the agency of its elastic walls, continually emptying itself by overflowing, through the capillaries into the venous system, overflowing at such a rate, that just as much blood passes from

the arteries to the veins during each systole, and its succeeding diastole as enters the aorta at each systole.

"It *cannot be too much insisted upon that the whole arterial system is over-full.* This is what is meant by the high arterial pressure. On the other hand, *the veins are* much *less full.* This is shown by the low venous pressure. The over-full arteries are continually striving to pass their surplus in a continuous stream through the capillaries into the veins, so as to bring both venous and arterial pressure to the same level. As continually the heart by its beat is keeping the arteries *over-full,* and thus maintaining the difference between the arterial and venous pressure, and thus preserving the steady capillary stream. When the heart ceases to beat, the arteries do succeed in emptying their surplus into the veins, and when the pressure on both sides of the capillaries is thus equalized the flow through the capillaries ceases."

A further examination of the text will more fully elucidate what I have been already compelled to extend to greater limits than I desire inasmuch as it would be an injustice to the author from whose work it is taken as well as to the writer under notice to abbreviate it to any great extent. Cursorily reading over this quotation there may appear to be some portions rather sustaining than refuting Dr.

Hammond's position, but a little reflection on the whole context will readily remove any such impression. Take for example the sentence: "When however sufficient resistance is introduced into any part of the course, the fluid being unable to pass by the resistance as rapidly as it enters the system from the pump tends to accumulate on the proximal side of the resistance." This of course is a plain hydraulic proposition, and would, taken by itself (and omitting all other considerations) indicate the possibility—no more—of the existence of such a disease as is under discussion, viz., Cerebral Hyperæmia. But it must not be taken by itself in this relation; the vital contractile power of the arterial coats, and the more yielding, less contractile condition of the venous structures on the other hand, are also to be considered; and, in addition, the fact that Dr. Hammond does not even attempt to prove or assert that any obstruction or resistance exists in this disease between the arterial and venous capillaries. In fact the disease, according to his own definition, consists solely and absolutely of an increase in the calibre of the smaller arteries. Now, it is self-evident that such a condition cannot exist longer than during one or two strokes of the pump, so to speak, without producing the old-fashioned active cerebral congestion, unless we presuppose an incurable disease of the arteries, which would give

Dr. Hammond some tangible "morbid anatomy" to instruct us upon, without indulging in flights of the imagination. The tension of the arterial system being largely increased must necessarily force on to their more yielding and expansive venous system the increased burthen; and when to this is added the peculiar disposition of the vessels of the brain, even, without bringing into calculation the hydraulic principle just alluded to, the enlarged calibre of the arteries, must necessarily almost immediately obstruct the venous circulation so as to cause passive congestion. Thus, in whatever light we view this problem, it is tolerably evident that the existence of this malady as a separate entity is not only not proven, but an utter impossibility.

CHAPTER II.

SYMPTOMS.

HAVING proven, at least to my own satisfaction, the absolute impossibility of "cerebral hyperæmia" existing *per se* in the form which Dr. Hammond claims, and names, accordingly I may perhaps justly dismiss any further discussion of the subject. But the reader will remember, that few if any of the medical authors of the country have achieved such prominence in public esteem as the writer under notice; that I had the boldness to state at the outset that this particular effort of his was remarkable for its number and variety of contradictions, and hence will permit me to strive to sustain my position by proof. I can scarcely conceive any form of labor more dull and dreary than poring over an uninteresting mass of medical literature; but when it comes to a tedious review, of a most flippant, disjointed, and contradictory work like that before us, I expect very few will have patience to follow it closely. To those who may I beg to say that the order is not mine, and that to appreciate "cerebral

hyperæmia" fully, it is essential to follow the writer in his own tortuous and devious paths. Let us take the first paragraph in this chapter on Symptoms: "That one of the primary effects of intellectual exercise or emotional disturbance is an increase in the amount of blood circulating through the brain, does not admit of a doubt except from those who, still refusing to learn, contend that the cerebral circulation is not subject to variation under any circumstances. Experimental physiology has, however, determined this point so positively in the affirmative that it is scarcely necessary to adduce the evidence in its support. It will be sufficient to recall the numerous facts observed by others and myself with reference to the *immediate cause of sleep*, by which it is shown that during the condition of wakefulness the quantity of blood in the brain is much greater than it is during sleep; the first being a state of *intellectual activity*, the latter *being one of* almost *complete cerebral rest.*" Although the connection between this dissertation and the "symptoms" is rather obscure, yet the paragraph as a whole is a very important one from the writings of Dr. Hammond, for this obvious reason that, carefully considered, he himself herein at once throws doubt on the accuracy of his deductions from those experiments which run like a refrain through all his writings, and for which he claims more credit than is

his due. Every reader must have noticed that if anything predominates throughout his writings—and they are manifold—it is that the bromides first produce a lessened quantity of blood toward the brain, and by this mode of action induce sleep; that *sleep in effect is a consequence of cerebral anæmia*, while in the paragraph above quoted there is an indirect admission that the *anæmia* is a *consequence* of *sleep*. Whichever of those theories be the correct one—though the matter is tolerably well settled—it is quite evident that Dr. Hammond's experiments—a faithful imitation of Mr. Durham's—fall decidedly short of proving his view, in face of such abundant testimony to the contrary. But the above admission is in strange contrast with what he so urgently insists on in his numerous contributions to medical literature, and it is well worthy of note on that account. This question will necessarily receive more attention later; but I may state at the outset that, while recognizing the condition known as sleep to be not only possible but probable in varied conditions of the brain circulation, the view embodied in the paragraph quoted I believe to be the correct one; viz.: that sleep, in the natural healthy condition, as far as it goes, is a cause of temporary anæmia, and that cerebral anæmia is not the cause—the sole cause, as Dr. Hammond would have us believe—of sleep. The analogy supplied by ordinary

physiological principles applicable to every part of the system, viz., that the more work the more change —the more waste; hence the greater requirement of more pabulum; hence the need of increased circulation—sustains this theory, and the evidence opposing it must be abundant and positive—which it is not—before it is abandoned.

The second paragraph, under the heading "Symptoms," has one singular *merit* in common with the first, *i. e.*, that nowhere does it touch on a single "symptom" throughout, but while the first recognizes approved and probable physiologic principles, the second completely ignores them, and wanders off into meaningless and discursive platitudes, *sic:*

"Excessive mental exercise inordinately augments the activity of the cerebral circulation. The blood vessels become over-distended, and if the brain be kept long in a condition of extraordinary action, they may be rendered incapable of returning spontaneously to their normal dimensions. Like a bladder filled to repletion with wind, they become in a manner paralyzed, and unable to contract upon their contents. They lose to a certain extent their elasticity, and, like the india-rubber band kept too long around a large bundle of papers, they do not regain their natural size even when the distension is removed. A state of cerebral hyperæmia is thus induced, which gives rise to a set of perfectly char-

acteristic symptoms, and which is fraught with peril to those in whom it occurs."

It certainly is not an easy matter to express any opinion of this "symptom" without losing to some extent that patience which is ever required in the daily routine of the medical profession; but I would ask, in the name of common sense what extraordinary action of the brain produces a *distension of the cerebral arteries from which they are incapable* of returning spontaneously? Apart from the healthy changes of the vessels unquestioned by physiologists, is such a condition of artery possible without the existence of aneurism or allied disease? Certainly not; and if this latter existed in "cerebral hyperæmia," surely the *raison d'être* of its nomenclature is a work of the flimsiest supererogation. In connection herewith, will the reader kindly remember the remark of Foster, the truth of which is self-evident:

"It cannot be too *much insisted upon that the whole arterial system is overfull.* This is what is meant by high arterial pressure."

Bearing this in mind, is not the whole of the above quotation perfectly devoid of meaning? and what other than ludicrous can be the simile touching the urinary bladder and india-rubber band? The latter being dead matter, whose normal condition is certainly not *overstrain*, may be readily

dismissed without a word, and very little also may be said of the urinary bladder. This viscus, whose function it is to allow of varied, indeed, very varied, distension, and unless the will comes into operation, up to a certain point makes no effort to rid itself of its contents, over which also the will has indubitable power, is contrasted with a vital tube whose function is of a totally different order, over which volition has no control whatever, and the calibre of which, if ever strained beyond the limit of its capacity of spontaneous recuperation, remains permanently injured! It is not easy to conceive a more flagrant fallacy in reasoning than this, and it becomes the manifest duty of every student of medicine to raise his protest against such thoughtlessness, no matter whence it comes, in matters of vital importance.

We are also told, in this paragraph on "symptoms," that this disease is accompanied with perfectly characteristic indications of its own; and what they are the author relates with as much calm indifference to previous writings and future criticism as though he were issuing an Ukase from St. Petersburg or a bull from Rome. When we come to examine them, however, as will be more manifest later, we utterly fail to detect what is "characteristic" about them, either separately or collectively.

It would be beyond the limits of this essay to

discuss the relative merits of Dr. Krishaber's views, as accounting for the certain train of symptoms which Dr. Hammond here asserts are due to "cerebral hyperæmia," but it is not too much to say that an effort in the right direction, a leaning towards philosophic medicine, seems to underlie Dr. Krishaber's mode of studying them, while the absence of all such leaning is conspicuous throughout Dr. Hammond's monograph.

For example, he tells us "The disease is sometimes developed with great suddenness, but ordinarily it advances little by little to completeness. When the former is the case, the patient experiences, under the influence of great mental excitement, pain in the head, vertigo, an inability to speak, or at least an imperfection of articulation. There are noises in the ears, flashes of light before the eyes, and occasionally for a short time double vision. The *heart beats with increased force and rapidity, and is more or less irregular in its action.* The *face is flushed*, and a feeling of suffocation is experienced. If he attempts to walk his gait is uncertain or staggering, not only in consequence of the vertigo present, but from actual loss of power in the limbs. Numbness is commonly felt in some parts of the body, and clonic spasms of the muscles, notably of those of the face, are generally present. With all these physical symptoms, there are others

indicating mental disturbance. Chief among these are hallucinations or illusions of the senses, particularly of sight and hearing. Insomnia is an almost invariable attendant, and what little sleep the patient obtains is interrupted by pleasant and even frightful dreams. Gradually *the disorder becomes established*, and the other functions, especially those connected with digestion, are deranged. From *the first the urine is loaded with urates* and phosphates."

The author has a strange facility for ignoring every argument that might suggest itself in opposition to his favorite theory. For instance, in this series of symptoms detailed here it may not be too much to claim that all the others could succeed to the violent heart-beats detailed, which may arise from any simple emotion; nor does it require a vigorous imagination to attribute the deposits of urates and phosphates in the urine to impaired digestion, which indeed, in turn, may account for all the symptoms reported. The curiosity of the whole, however, is that few people beside the author himself will be able to recognize wherein lies the "characteristic" features of the symptoms, for it may be very safely asserted that any, and all of them are quite as "characteristic" of cerebral anæmia as of hyperæmia, and indeed may exist, in their most marked form, without the presence of either disease.

Of the correctness of this statement the reader will find abundant proofs in Dr. Hammond's own work on the "Diseases of the Nervous System" (article Cerebral Anæmia), and in Trousseau's classic "Clinique Medicale (lecture) "Vertigo a Stomacho Laeso." In view of the importance of this subject, it is scarcely justifiable to garble the masterly lecture of Trousseau; but a few brief quotations will suffice to indicate how strongly he felt relative to the mistakes that may arise in this direction; how accurately he observed morbid conditions; and how very much in accord with what we commonly note his teaching is:

"La tenacité des phenomènes qui le caracterisent, ses retours incessants, son apparente gravité, tourment étrangement les individus qui en sont affecté, en imposant aux personnes qui les entourent, et souvent même aux médecin, qui ne connaissent sa nature, peuvent instituer pour le combattre des médications diamètralement opposé a ces qu'elles devraient être.

"Combien de fois, en effet, ces vertiges stomacaux n'ont ils pas été mis sur le compte de *prétendues congestion cérébrales* contre lesquelles on essayait de lutter par un traitement anti-phlogistique, par les saignées, les applications des sangsues, les purgatifs, la diète rigoureuse; et ses moyens, aux lieu de guérir le mal, en augmentaient l'intensité tandis

qu'un régime convenablement réparateur, un traitement tonique en auraient en promptement raison."

"Mais la forme que vous rencontrez le plus ordinairement est celles qui a reçu l'épithète *gyrosa;* quand l'individu est debout, tout tourne autour de lui; il est obligé de fermer les yeux et de se tenir dans la plus complète immobilité, car il sent des jambes vaciller, fléchir sous lui, il va tomber et tombe même quelquefois."

In an illustrative case he says:

"Cette douleur avait ceci de singulier qu'elle se calmait spontanément aussitôt après le repas, mais alors aussi le malade éprouvait des phenomènes cérébraux qui le préoccupaient singulièrement, c'étaient des *étourdissements des troubles de la rue*, des *bourdonnements* d'oreille, une *pesanteur de tête*, un sentiment *qu'il comparait* à celui que *procure l'ivresse a sont début.* Il affirmait qu'il ne buvait habituellement avec excès ni vin, ni liqueurs. S'il essayait de se lever de table, ses jambes fléchissaient sous lui, et il resentait une malaise générale, il lui semblait qu'il allait s'évanouir; pour ne pas tomber il était forcé de s'appuyer sur ce qui se trouvait à sa porte. Jamais cependant il ne perdit connaissance."

To this he adds:

"A lady at a certain time of life comes from Paris to Bordeaux, to consult me about brain symptoms which have troubled her for several months, and

which consist mainly of forms of vertigo that scarcely give her any repose. On the most trifling occasions they manifest themselves, and go so far as to produce a state of syncope that enforces the recumbent position. Motion on the street, the sight of persons passing by, of a carriage moving a little quickly, brought on a return of the attack, so that in a short time the lady was unable to leave her bedroom. She believed herself threatened with apoplexy, and her fears were increased by her unhappy surroundings that in a measure tended to foster this idea. To avoid the congestion that she dreaded according to the theory advanced she pursued a system of rigid dietary—limiting herself to gruel and soup—through fear of increasing her blood. Her appetite diminished, but her digestion in general remained tolerably regular.

"Some medication and a sustaining and nourishing dietary at the end of eight days brought about a notable change in the situation. The attacks of vertigo were much less frequent, and in a short time the patient was entirely rid of them. Six weeks later her natural rotundity and health returned."

If the interested reader can detect any intrinsic difference between the train of "symptoms" that (as Dr. Hammond asserts) characterize "cerebral hyperæmia," and those described by Trousseau as

accompanying ordinary stomach disturbance (in some people), I shall heartily accord him a capacity for discernment that has not been given to me.

Compare the first case under the heading "symptoms," by which Dr. Hammond illustrates the presence of "cerebral hyperæmia" with the first case quoted above from Trousseau. Translate the one into French and the other into English, and as far as the essentials of illustration are concerned, and as far as relates merely to "symptoms," either may serve the purpose of one writer or the other.

"F. H., a gentleman engaged in a manufacturing business which required all his attention to make it profitable, was informed one morning by his superintendent that a large lot of material had been spoiled. He at once experienced an intense sensation of vertigo, a sharp pain in the head, palpitation of the heart, and would have fallen had he not been supported by the bystanders. There was also a roaring sound in the ears and flashes of light before the eyes. On attempting to stand, the vertigo and palpitation were increased. There was at no time loss of consciousness, though the ideas were confused and the speech thick. In the course of a few hours the severity of these symptoms diminished, but that night he was unable to sleep, and in the morning the morbid phenomena reappeared, but with diminished violence. For several months afterwards he

was troubled with wakefulness, a sense of fulness and tightness in the head, occasional weakness of the limbs, a slight numbness, and a total inability to exert his mind in his business affairs without an increase in all the symptoms. *Under appropriate treatment he entirely recovered.*"

This case of F. H. is so essentially like that of Trousseau's hospital patient, that if clad, as I have said, in the same garb of language, it would be difficult to tell them apart. The question then naturally arises, among such diametrically opposed views, which is the correct one? Dr. Hammond refers this form of trouble to brain hyperæmia; Trousseau would refer the same to a disordered stomach, with probably a leaning to the view that the sympathetic system is more or less implicated—and which guide are we to follow? The practical physician, who is not a specialist, will have observed numbers of cases of this nature, and will formulate his own views accordingly. To myself the answer is simple enough, believing as I do, without further proof, in the impossibility of existence of such a condition as the "cerebral hyperæmia" of Dr. Hammond.

To those, however, who may be inclined to adopt the brain theory (so to speak) in this connection, I would suggest to observe the effect of shock in its various manifestations on different constitutions. By it one's breathing is suddenly arrested;

another falls down in syncope; a third vomits; a fourth becomes a lunatic; a fifth has jaundice; each, according to some constitutional peculiarity, has a distinct and separate disturbance from what may be to others again a source of no evil whatever. I would further remind them of the fact that Trousseau very plainly tells us how he restored his patient's health—by tonics and substantial fare; while Dr. Hammond forgets to inform us of anything more definite than "appropriate treatment," by which, indeed, if we follow the later instruction of the author, we may reach the very opposite poles of the pharmacopeia.*

To follow the author into details is as difficult as unprofitable, and the more care devoted to the pursuit the more manifest this assertion becomes. For example, after having recited two more cases like that of F. H., he deliberately tells us "These cases are cited not as exhibiting perfect representations of cerebral hyperæmia, but merely for the purpose of illustrating the suddenness with which the condition may be induced."

* When we have recommended such diametrically opposed plans of treatment as that of bromide of potassium on the one hand, and phosphorus, nux vomica, etc., on the other, it is high time to ask which has been adopted in any given case, or at least our current notions of the therapeutic action of those drugs should be corrected by careful and unimpeachable experiments.

It would appear then that in this disease, which, later on, we are gravely informed is merely a prelude to more serious mischief, there are various stages, and yet to the less instructed than Dr. Hammond it would seem that a mere prelude to other diseases, like the one in question, and that, too, in a matter of plain, tangible anatomy, which has lasted over twelve months, like some of the cases recited, ought to be pretty perfect of its kind—pretty well developed, as far as such development is possible.

This is merely pointed out as perhaps the culpable carelessness of expression, which the author, in justice to himself, and the profession he would instruct, should make an effort to correct in future editions—if such be demanded—of this most novel work.

As far as warrantable, now, I shall endeavor to avoid minutiæ and endeavor to present the writer's views in full. We are told that the symptoms of this new disease are broadly: *Wakefulness*, *Illusions*, *Hallucinations*, or *Delusions*, *Mental introspection*, *Vertigo*, *Heat*, pain, and a sense of fulness in the head.

On carefully comparing those symptoms either separately or collectively, it does seem a little odd that any author should base on them the fabric of a new disease, seeing that they are not at all uncommon in diseases with which we are more or

less familiar. What there is "characteristic" about those symptoms seems, to say the least, a little obscure. What febrile condition do we know of that in its inception is not commonly accompanied by this same train and chain of symptoms? Take an acute case of hepatitis or gastritis, or even nephritis for example, and has it not in many subjects all the so-called "characteristic" symptoms of "cerebral hyperæmia?" In the commonest disease of our climate of California, which for want of a better name we call catarrhal fever—a general febrile disturbance arising from cold—what do we more commonly observe than "wakefulness, illusions, vertigo, pain and heat," etc., and are not these the exact indications of its presence, without any further positive evidence? But it will be urged that the complete combination of the symptoms mentioned is the characteristic feature of the disease under consideration. In answer to this I would simply ask the reader to glance over the illustrative cases in Dr. Hammond's own work. Again it may be alleged that the diseases I refer to produce cerebral hyperæmia. This I readily concede, and also recognize this form of cerebral hyperæmia. But what would be the result if in either of those diseases our treatment were solely or even primarily directed to the cerebral disturbance? Suppose in a common bilious attack, as the popular expression goes—and the nomenclature is

far from misleading in the sense that more grandiloquent ones are—which has all the "characteristic" symptoms above quoted, we commenced by pouring in bromide of potassium as the chief instrument in reducing blood-pressure to the brain, what would be the result? Only intense aggravation of the malady, as far as we can judge from the common action of the drug.

And thus we may go on recounting and culling from various authors diseases of the most divergent origin and course to which all those symptoms are common. Not only will they be found in Dr. Beard's work on Neurasthenia—the counterpart of Dr. Hammond's cerebral hyperæmia—but they frequently appear in monographs on uterine diseases, and indeed among the works on the male genital organs also they familiarly obtrude themselves on our notice. I must not be understood, however, as implying that under those varied circumstances they exist merely in the imagination of the writers. On the contrary, I am convinced that this series of symptoms will be found in any form of malady producing grave constitutional disturbance no matter from what cause arising. But while it is one thing for an author to point them out as existing in relation to the special subject of which he treats, it is quite another proceeding when a prominent writer asserts that they are "*characteristic*" of an entirely

new order of things, which on this basis alone he moulds into a new and distinctive name.

To enter now on a little more detail: "Wakefulness," we are informed, "is generally the first symptom of cerebral disturbance, and this may be of all degrees from the loss of an hour or two of sleep to the passing of night after night in a state of complete insomnia. Even if sleep be obtained in some cases it is disturbed by nightmare and frightful dreams, and the patient accordingly rises feverish, unrefreshed and entirely unfit for mental or physical exertion." After this we are treated to various illustrations of the direful effects, that follow, on the emotions, memory and judgment.

For my own part I have little doubt that wakefulness unduly protracted leads at length to very deplorable results, although I have known hysterical ladies, otherwise healthy, whose maximum of sleep has not exceeded an average of more than two hours in twenty-four for years in succession, and I am acquainted with professional gentlemen whose capacity* for mental toil is unsurpassed, who assure me that three or four hours is as much as they can sleep any night for weeks, and even months at a

* One of the ablest lawyers in the United States, who, recently in a noted criminal trial, displayed his pre-eminent ability, assured me that for *one hundred and fifty* nights prior to this trial, he never retired to bed before 3 A.M., and breakfasted between 8 and 9 A.M.

time, and this without the slightest impairement of their intellectual vigor. But the question here, is, whether "wakefulness" be due to cerebral hyperæmia or not. Now on this question there is much information to be derived from the views of alienist physicians, and among them there is a rare concurrence of opinion that this symptom of insomnia is one of the first and most frequent indications of approaching insanity, but none claim that insanity is due to cerebral hyperæmia. On the contrary, there is a tolerable unanimity of opinion among this class of observers, and statistics tend to prove it, that the hydra-headed malady of this name is commonly the offspring of brain fatigue and malnutrition; of destitution and want; of irregular habits and exhausting toil, combined with the strain that a precarious mode of living, naturally produces on an enfeebled nervous system. Nor is insanity by any means the only malady of which wakefulness is the precursor. Who has not observed it in typhoid fever before the disease had fairly set in? who has not noticed it in that painful scourge, acute phthisis, even when no other indication existed, save perhaps an occasional night-sweat? But it is unnecessary to multiply examples of this nature, and I shall therefore pass on to a very curious illustration of the strange pranks to which "hyperæmia" can subject the human judgment.

"I have known," Dr. Hammond tell us, "a patient suffering from *the disorder in question*, take several thousand dollars every day for a month or more to Wall street, fully resolved when he left his house how to invest his money, but always returning with his purpose unfulfilled—a prey to doubts for which he could give no sufficient cause even to himself."

This gentleman's case may, indeed, be a very curious study, but wherein the certainty of his condition depending on "cerebral hyperæmia" lies, I am at a loss to understand. Under the light of recent brain physiology, however, it is quite intelligible that while at home, *i.e.*, before leaving his house, some surrounding circumstance may invariably produce a hyperæmia of that portion of his brain that presides over the passion of avarice, and when on Wall street, the wrecks strewn around or other noticeable objects, may produce an ebb in the *avarice* tract and a flood in the direction of the *caution* region. Possibly this same theory of hyperæmia may explain how an esteemed professional friend missed a fortune!

In discussing the question of "free will," on one occasion to sustain his negative view, he remarked, "Now I held such and so many shares of mining stock, for a long time, and paid assessments faithfully for years. One morning something disagreed with me at breakfast, which gave me a mis-

erable attack of nausea, and a horrid fit of "the blues" in consequence. In my unpleasant frame of mind, I went to my brokers and ordered all my stocks sold that morning. That same day stocks began to advance, and in a month from then I should have been a millionaire if it were not for that accursed breakfast."

Applying this course of reasoning generally, how readily it may be argued that stomach-hyperæmia has as much to do with the emotions and judgment as cerebral hyperæmia has; nor is the assumption inconceivable considering our very limited knowledge of the great sympathetic nervous system.

However, to enter even briefly on a discussion of the effects of external conditions on the mind is no part of my object at present, no matter how alluring this subject may be. My task is simply to endeavor to establish the fact that gentlemen in our profession, even of the most colossal reputation, are not to be followed blindly without scrutiny of the ideas they advance; and, I make bold to say that in the brochure before us, the reader, who is at all interested, will admit that the author is far from advancing professional knowledge.

I think it is no exaggeration to say that the chapter on symptoms, by no means the worst of its contents, would alone entitle this work on "cerebral hyperæmia" to a prominent place among

the "curiosities of medical literature." Take the following: "The emotional system participates in the general mental disturbance, and indeed, is often the part of the mind most prominently deranged. The passions are easily roused into activity by slight exciting causes; trifling circumstances produce great annoyance, and the little every day troubles of life appear of vast importance. The disposition accordingly becomes suspicious, peevish, fretful. Persons thus affected are very far, ordinarily, from being pleasant companions. Many of them avoid social intercourse, and shut themselves up in their rooms to brood over their real and imaginary disorders. Others, again, plunge into dissipation and excesses of every kind, in the vain expectation of being able by such means to overcome the disease; and, again, others strive by a constant change of one physician for another, or substitution of one quack medicine for another equally quackish, to get relief from their mental and physical distress. In some there are very few decided symptoms present, except the inability to sleep and the incapability of concentrating the mind on any object of study or labor, without inducing pain or discomfort of some kind in the head. *In all, however, there is the same mental introspection.*

If this be not an excellent description of our old familiar enemy, hypochondria, pure and simple,

without even a change of dress, English phraseology can bear more widely divergent interpretation than I am familiar with*; and if Dr. Hammond can demonstrate that this most intractable malady is due to "cerebral hyperæmia," mankind and the profession for the centuries to come would bless his memory. But alas! this theory has been tried and found sadly, woefully defective; and the physician at the present time who would strive to subdue hypochondriasis by those mighty subduers of "cerebral hyperæmia"—the bromides—had better have a care that their patients cannot at all times lay hands on dangerous weapons. Although the question as to whether hypochondria—and even Dr. Hammond cannot refuse the name to the symptoms described—depends on "cerebral hyperæmia" or not, scarcely admits of serious discussion, yet it may be proper to see how other authorities view this obscure malady.

Sir William Gull and Dr. Anstie, whose laborious efforts and general ability few will dispute, in describing its etiology, make use of the following language: "The 'causes of hypochondriasis' is an expression even more singularly unhappy than the

* In order to avoid further quotations than strict necessity demands, I would refer the reader to the article on hypochondriasis in Reynolds' "System of Medicine," where he will find this same description applied to the diseases discussed therein.

average instances of a phraseology of causation applied to those circumstances which preceded the outward and invisible development of functional disorders.

Again it is commonly said that reading or conversation on medical topics often frightens laymen, and more rarely even doctors into a nervous and hypochondriacal frame of mind. There is, doubtless, something to be attributed to such influences, but *the most thoughtless person could not fail to be struck on reflection with the surprising infrequency of hypochondriasis in comparison with the ubiquitous operation of such influences as grief, fatigue, the sense of shameful failure, the habit of miserable and heart-wearying idleness*, and the practice by the laity of reading medical treatises."

Again, concerning the pathology, they say:

"The pathology of hypochondriasis in the strict sense of the word does not exist, for there are no anatomical or physiological facts upon which it can be based. *Morbid anatomy has revealed absolutely nothing* which in the slightest degree explains the occurrence of the disease; and the physiology of the symptoms is to the last degree obscure and uncertain in its interpretation.

"It is neither impossible nor unlikely that the improved modern methods of examining the nervous centres, if they could be applied to the central

ganglia of certain visceral nerves (and especially to the nucleus of the vagus) might detect appreciable changes even in the early stages of the disease."

Bearing in mind that the symptoms above quoted from Dr. Hammond's work coincide almost verbally with those detailed in Gull and Anstie's articles referred to, the reader is called upon to adopt the pathology suggested by the former, or accept the "unvarnished tale" of the latter gentlemen. To my mind the undiscovered and impossible pathology of Dr. Hammond only proves to what extravagance "hobbies" can lead the individual in any calling, who is pampered by an unmerited popular *reclame*.

If the reader for a moment imagines that I am the advocate of any special theory, or have other motive than a common sense of justice in this matter, I would beg of him to read this monograph on "cerebral hyperæmia," from pages 27 to 32. I doubt if in the English language there be a more succinct description of all the symptoms of hypochondria, and yet we are blandly informed that they are the "characteristic symptoms" of the "new" disease.

Again: "Vertigo is, however, the most prominent of all this category of phenomena in the majority of cases, and may be so severe as to *prevent the patient moving about*.* In one case recently under my

* See prior quotations from Trousseau.

charge, the subject, a gentleman of about forty years of age, was often seized with intense vertigo while walking in the street, and was obliged at such times to seize hold of a lamp-post, or if this was not within reach, to sit down on the nearest door-step, or even the curb-stone, till the violence of the attack had in a measure abated."

Thus we are carried along in the loose category of "characteristic symptoms," without even a pause being made to enquire as to the possibility of their arising from totally different causes or not. An unreasonable theory is upheld by a profusion of meaningless verbiage that mystifies the reader at every step, and the unfortunate practitioner who unfalteringly follows the dicta laid down in this singular monograph, will too late discover to what absurdities he is so unhesitatingly led.

VERTIGO, the most *prominent symptom* of "cerebral hyperæmia," of what exhausting disease or condition is it not a symptom? Is it not one of the foremost symptoms of uterine diseases as well as that under discussion? When we pass an instrument to relieve a stricture of the urethra, and the individual topples over half conscious on the floor, must we at once conclude that this direful cerebral hyperæmia has set in? When an individual with disease of the uritral valves tells us that he felt so faint as to be obliged to lean against a door or lamp

post, else he would have fallen on the streets from *vertigo*, are we to recognize at once enlargement of the brain arteries, and deplete them accordingly? When we open a felon in the hand of a poor, emaciated domestic, and vertigo ensues, must the bromides be immediately called into requisition, and her digestion deranged in addition to her already existing discomfort? This "characteristic symptom," in short, I find so commonly existing among diseases of all characters and kinds, that I am totally at a loss to understand how it can be claimed in a modern work on medicine as peculiar to any one disease.

Not only is it a common characteristic of diseased conditions, however, but it is also quite compatible with an excellent degree of health. What surgeon, leaning for a long time over some tedious operation; what student, sitting at his desk for several hours devoted seriously to some common mental labor, has not occasionally experienced more or less vertigo on assuming the erect position? And in this connection, bearing in mind ordinary hydraulic laws, I would ask the physiologist whether cerebral anæmia, under such circumstances, be not a more probable cause of vertigo, *i. e.*, if the vertigo be at all governed by the condition of the circulation. Considering my previous remarks about "cases," it is not quite consistent to introduce any of my own, but I trust

two very marked cases of vertigo, which have come under my notice, will not be here out of place, the more especially as I feel assured they accord with the experience of every intelligent practitioner whose attention is at all turned to the strange phases of nervous diseases.

One is that of an eminent member of the bar of this State, who for several years had been the slave of alcohol, but otherwise of an uncommonly robust constitution, physical and mental. Through the efforts of a powerful will and some slight aid on my part, he abandoned his besetting vice several years ago, but for some two years after he frequently reported himself as afraid of *falling* on various occasions; sometimes in court, at other times on walking along the street. During an absence on my part of some six months in Europe he consulted an intelligent gentleman unacquainted with his previous history, and the very first advice he received was to indulge in "a moderate quantity of wine and stimulants." He did so, and immediately his "vertigo" disappeared, but feeling that this course was injudicious he again abandoned stimulants, and again his vertigo returned. As on the former occasion, again all alcohol was withheld on my return, and the patient was put on a free use of valerian, nux vomica, ammonia and calisaya bark—all of which as far as we know their therapeutic value—tend to produce cere-

bral hyperæmia, and yet this gentleman now, after a lapse of six years, not only survives in the flesh, free from the dreadful consequences Dr. Hammond so eloquently depicts, but stands forth as an ornament of his profession, alike remarkable for his assiduity and talent.

Another case is that of a gentleman about fifty years old, whose ample fortune suddenly disappeared in one of the financial collapses to which we are so periodically subjected. He is known to all his acquaintances as a man of the most imperturbable character, having been more than once sorely tried in most trying situations. He is probably the nearest approach to the "man of iron nerve," it has ever been my fortune to meet, and yet on more than one occasion of late he has fallen unconscious on the street. On the last occasion the attack lasted such a length of time, that he found himself in the receiving hospital of the police department, without knowing anything as to how or wherefore he arrived there. On his consulting me after this, I concluded that his malady was some of the obscure forms of epilepsy, for the reason that all his organs appeared to be in the most excellent condition. Other illness in the family, however, shortly after led me to visit the house, and from the revelations then made, I readily changed my opinion. I found that the proud man had been for some time

living in a condition to which he had all his life-time been a stranger—that meagre and even scanty fare had taken the place of generous and sumptuous living. A worthy friend by suitable occupation relieved the "res angusti domi," and now for eighteen months there has been no collapse on the street; no vertigo. Was this, I ask, a case for depleting the cerebral arteries; for these all-powerful bromides that nauseate the stomach; or for abundant and nutritious diet?

So much for "vertigo the most prominent of all the symptoms." Let us now glance at the subject of illusions where we find—

"The special senses could scarcely be expected to escape giving evidence of derangement, and hence among the chief manifestations of the intra-cranial disorders are those connected with the perceptive organs. Thus there are noises in the ears, such as roaring, rumbling or singing, and occasionally loud reports such as might be produced by the discharge of firearms. A gentleman recently under my care suddenly heard a report as if a pistol had been shot off within a foot of his head. He jumped to his feet, expecting to see an assailant behind him, but to his surprise there was no one to be seen, and it was very evident that no explosion had taken place. After this there was scarcely a day that the sound was not heard. It was entirely subjective, as per-

sons in close proximity to him at the time heard nothing."

If a novice in medical literature had published this sort of twaddle, the probabilities are that all the journals of the country would so load him with ridicule that his name would be never heard again. In the name of common sense, so frequently outraged in this precious monograph, why not first prove that illusions in general are dependent on hyperæmia, and then proceed to illustrate it by cases? But what medical observer at the present time doubts that cerebral anæmia is not a more frequent cause of illusions and hallucinations than cerebral hyperæmia. Witness the mental condition of our patients in any exhaustive disease, and above all follow closely the asylum records of confirmed insanity of one kind or other, and it will be found that physical exhaustion and hardship, and malnutrition, are the great factors in filling up the homes for the insane throughout the world. Apropos of this subject of illusions, how does the writer explain their constant presence in *delirium tremens*, and their complete abolition by such hypnotics as opiates and chloral hydrate, which we know—if we can rely on anything as definite in therapeutics—produce the utmost afflux of blood to the brain.

This form of inconsistence, however, is so frequent and marked that it would be ludicrous were

it not for the gravity of the subject. For example, in the page next to the quotation above given, we find:

"In another case, the onset of the disease in a gentleman who had for many years overworked his brain, was extremely sudden and was attended with facial paralysis. I treated him for this latter condition *with electricity*, with but little benefit; but one day he struck his head violently against a gas burner hanging over his desk, and shortly afterward felt something give way within his head, with a sharp, snapping sound, and the paralysis instantly disappeared, after having lasted some five or six days."

This is exquisitely droll, and the writer must have, at least, a fine vein of humor in his composition, for the advices implied herein are—that electricity, which he so constantly recommends in cerebral anæmia, can also be turned to relieve cerebral hyperæmia; and, 2d, failing in this, we are to try the efficacy of a first-class bludgeon on the obdurate head of the patient, for a good hickory may, without doubt, be said to lay claim to as much virtue in this direction as a mere commonplace gas burner.

After this we are naïvely informed that "such cases are inexplicable," although the while they are all given as illustrations of "cerebral hyperæmia," and the reader must then form his own conclusions

as to how a writer, who thus so abruptly contradicts himself, has ever achieved any higher position than a mere butt for ridicule.

There surely must be some "brain bankruptcy" in the immediate neighborhood of an effusion that in one breath claims that a certain condition is the result of "enlargement of the cerebral arteries," and immediately after claims that the condition is "inexplicable."

I shall not attempt to follow the writer through the various mazes of his aural and ophthalmoscopic investigation, but it is not too much to claim that all the conditions detailed by him as existing in those organs are found abundantly in the diseases peculiar to these parts without any brain disease whatever, and are as frequently missing in the cerebral diseases most closely allied to this alleged new one.

Among the symptoms, after those above referred to, are given: "*Disturbance of the sense of smell and taste; sensation and the power of motion; the appetite; constipation and the urinary secretion.*" Why not add club foot or hernia?

In view of the paragraphs already taken from Trousseau and Foster, however, the remarks on appetite and urinary disturbance are quite interesting in their way.

"The appetite is capricious, and the stomach acts

imperfectly and sluggishly. The gastric juice is not secreted in sufficient quantity for the purposes of digestion, and, the peristaltic action of the stomach being weakened, the food remains within it a long time undigested and undergoing fermentation. Regurgitations both of the solid contents and the gases are common, and the patient tastes his meals several hours after they have been swallowed. Gases accumulate in the stomach, and give rise to the sense of fulness experienced even after a very slight repast has been taken. Such symptoms are usually classed under the name of "nervous dyspepsia," a not improper designation if it does not lead to the error of regarding them as of primary importance, instead of considering them as they are merely consequent on the head trouble."

Now given the conditions here detailed in a patient under notice, I ask the reader not carried away by any wild theory of an alleged new discovery, whether he would direct his primary treatment. I ask Dr. Hammond himself whether he would treat a person thus suffering with nauseating bromides to dispel presumed brain trouble, or treat the positive manifest disturbance of the stomach with tonics and light nutritious diet? Every reader will recall many cases of this nature in which the "*stomacho læso*" theory advanced by Trousseau has left nothing to desire, and in which, if the brain

were the point of attack so to speak, failure would undoubtedly result.

"The urine is in some patients scanty and high-colored; *in others it is profuse and almost as pale as water.* Oxalate of lime is often present, and an *excess of phosphates* an *invariable* condition, so far as my experience extends. I have already spoken of this circumstance. *Whether or not the phosphates in the urine are to be regarded as the ashes of the nervous system, and hence a measure of the amount of nerve tissue decomposed, there is no doubt that they are inordinately increased after intense mental or emotional strain.*"

After what has been already said against this theory, that the amount of phosphates or salts in the urine bear any proportion whatever to the amount of brain labor, a further discussion of the subject would be out of place here. It is, however, interesting to observe that what was positive assertion on a foreign occasion (*vide* introduction) is here thrown out with a judicious modicum of hesitation by the writer.

Tedious and profitless as it may be, I would ask the reader to give some consideration to the following, also among the "characteristic symptoms":

"But one of the chief categories of symptoms remains to be considered—chief at least so far as the more obvious appearances go, though like the other

visceral derangements, I must regard these as being due to the brain disorder—and that is the group of phenomena connected with the heart. To Krishaber, in the work already cited, belongs the credit of being the first to call attention to this remarkable series, for in the publication of my own to which I have referred, it was in a great measure overlooked. As Krishaber remarks, the troubles of the circulation consist especially in an irritability of the vascular system, so that the least movement, such as rising erect from the sitting posture, or to the sitting from the recumbent, leads to an acceleration of the pulse of from twenty to thirty or even forty beats to the minute. Besides this, there are frequent and violent palpitations, either spontaneous, or provoked by the most insignificant causes, mental or physical.

"*Emotional excitement is, however, the most prolific cause of cardiac disturbance in patients affected with cerebral hyperæmia*, and at times leads to serious results."

The least that can be said of this meaningless method of dealing, with a subject of extreme professional importance, is that it is reprehensible beyond measure. What human heart is not disturbed by "emotional excitement" in a greater or less degree, and are not all the human family sufferers accordingly from "cerebral hyperæmia" according to Dr. Hammond? But the little real consideration given

to this whole subject by the writer, is probably herein more apparent than in any paragraph of similar length in the work. Surely a gentleman who parades before the nation as an authority on nervous diseases, cannot be ignorant of the fact that the sympathetic nervous system—not the brain—is universally regarded as the motor power of the heart, *i.e.*, as far as experimental physiology can elucidate the subject, and consequently the assumption that hyperæmia of the brain can be the cause of cardiac disturbance, is entirely without foundation; while, on the other hand, it can be easily understood how readily cardiac disturbance can give rise to such form of cerebral hyperæmia as is possible. It is unnecessary to remind the reader that the sequence would naturally be—1st, emotional disturbance; 2d, cardiac excitement; and 3d, cerebral hyperæmia—always denying that this condition described by Dr. Hammond has existence other than in his imagination.

I presume another "characteristic symptom" is intended by the following: "If the disease be not arrested, it is quite certain to develop into one or another of the forms of *cerebral congestion, apoplectic, paralytic, epileptic, soporific, maniacal or aphasic*, to which in another place I have given full consideration. I do not dwell on them here for the reason that this essay is concerned only with a series of

phenomena which I conceive to be due to cerebral hyperæmia, constituting the *first, or prodromatic,* stage of congestion of the brain."

This final paragraph on "symptoms," though by no means unique in the work, baffles all criticism, for its ring and clamor is only worthy of the street vender of patent medicines. I would not worry the reader by contrasting the various epithets above applied to different forms of cerebral hyperæmia. In face of all authentic facts to the contrary, established on a basis as secure as anything in the positive sciences, he jumbles together a string of words, and would have us believe that such widely different diseases as *apoplexy* and *epilepsy*, *aphasia* and *mania*, have a common origin, and then, with a boldness that is positively unrivalled in the legitimate ranks of the profession, wheels round and informs us that this new disease—which on a former occasion he informs us lasted in some cases over twelve months—is merely "the prodromatic stage of cerebral congestion." This, undoubtedly, it must be, whenever it has any existence, and, as a prodromatic stage, cannot last longer than two or three beats of the heart at most.

CHAPTER III.

"DIFFERENTIAL DIAGNOSIS."

THE opening of this chapter, not unlike much that precedes it, must be curious reading to the thoughtful practitioner of medicine, and while in the main feature of inconsistency it is not materially different from the others, yet, if anything, there seems to be more *abandon* displayed throughout as the writer progresses. Thus: "Although the main features of 'cerebral hyperæmia,' such as I have described them, have been to some extent known, they have heretofore been ascribed to very different pathological conditions. Thus they have been classed under the heads of *nervousness*, *chlorosis*, *malarial disease*, and even under the very opposite condition to that to which I assign them, '*cerebral anæmia*.'"

In this category it is a little surprising that "hypochondria," the description of which so closely conforms to that of the new disease, has no place or mention, and that the vivid portraiture by Trousseau of "vertigo a *stomacho læso*," which is not a

dyspepsia pure and simple, and which in the main so closely resembles "cerebral hyperæmia," has also been completely ignored, though this form of malady is probably one of the most frequent with which practical men have to deal. Nor is it less surprising that "*chlorosis*" and *malarial disease* should have a place herein, considering how distinct their features are, compared with the symptoms already given of the "new disease." But more of this anon. To continue the quotation:

"The name by which we designate a disease *is of no consequence so long as it does not lead us into erroneous ideas of treatment.* But *it is rarely the case that this can be avoided,* and I am very sure injury has been done in the nomenclature of the affection under notice. It is *important, therefore, to have exact conceptions of the character of the symptoms and the general course of the disease before we venture to give it a name.*"*

The truth of this statement throughout no rational physician will dispute, but how closely Dr. Hammond adheres to the fundamental idea, the reader must be the judge, both from what precedes and follows. What "exact conception" of "cerebral hyperæmia" can be obtained from the series of

* It may also be essential to have some *faint idea* of the pathology of the disease.

platitudes contained in this disjointed and diffused chapter on "characteristic symptoms"? It is no exaggeration to say that there is nothing characteristic in it, that all the symptoms detailed therein are common to many diseases, two especially: hypochondriasis, and "vertigo a *stomacho læso*." That injury has been done, and will continue to be done, by this unique monograph, and especially in consequence of the author's prominence, needs no proof whatever on my part; but if such be needed, it is necessary only to bear in mind what the writer dwells on with most force throughout, though not expressing it in so many words, viz.: that nearly, if not quite, all the obscure diseases to which flesh is heir (see chapter on symptoms) arise from "cerebral hyperæmia," and that the remedy, *par excellence*, for this condition is one or other of the bromides. It is true, however, that in detailed instructions these two leading principles are constantly departed from, and so somewhat foiled in their viciousness.

To further show that I do not exaggerate in stating that the author ascribes to "hyperæmia" such multitudinous diversities of neurotic troubles, let us take the first case in this chapter on "differential diagnosis":

"Many of the so-called menstrual neuroses are, in reality, cases of cerebral hyperæmia, the result

of inordinate emotional strain. To establish this point it is only necessary to consult the collection of cases made by Berthier. Thus, for instance, case 34, under the head of hyperæsthesia, is clearly an almost typical case of 'cerebral hyperæmia,' and is as follows: 'Jeanne Thillois, aged 51 years, was of strong constitution and had never been ill till at about her thirty-sixth year a violent mental emotion caused a suppression of the menses, and this was followed by heaviness of the head, wandering rheumatic pains, then by weakness in the arms, legs, shoulders and scalp, and by attacks of vertigo of so intense a character as to cause her to fall to the ground. A year later there were headache, delirium, weakness of the limbs, and pain along the *whole length of the spinal column.* She was treated with the cautery and blisters to the skin. With time there was a gradual amelioration of the pains, but the weakness of the limbs, troubles of speech and the difficulty of deglutition persisted' (Berthier). Many cases similar to the foregoing might be readily collected from Berthier, and nothing is more common than for just such mistakes to be made in regard to cause and effect as in the instance cited."

This case is recorded no doubt to show mistaken diagnosis and improper treatment. But the proof of both! what doth it consist of? Merely the dictum, the unsupported assumption of a writer who

publishes a work on an unrecognized disease in medicine without a solitary fact to sustain his position in this regard; indeed, it might be said, publishes a work which is a paradox in itself. This method of dealing with the subject—of annihilating dissentient opinions, is not, however, new to the reader, and the only inference left for him in the above is that, if the infallible bromides had been poured into this unfortunate, her speech and deglutition and other faculties would be peremptorily restored—perhaps in *ten days!* Considering his method of reasoning —and it would be only a natural sequence of the past—it would not be at all surprising to hear of Dr. Hammond next expounding a new system of inductive philosophy, for assuredly he bids a persistent defiance to all our received notions on this subject; and Aristotle and Bacon must pale before the new "*lux major.*"

In reference to the subject of *chlorosis*, the tone of the work is progressively displayed, without stint, and possibly introduces one new feature in the author's method of displaying his singular facility for falling into his own trap. It will be remembered that *chlorosis* in this chapter on "Differential Diagnosis" is one of the maladies to be carefully distinguished from "cerebral hyperæmia," but, like dyspepsia *et id genus omne*, it now falls into line, and becomes a part and parcel of the great progenitor of all human

ills—that *fons et origo malorum*—"cerebral hyperæmia."

"The affection known as *chlorosis* occasionally presents features similar in some respects to cerebral hyperæmia, and indeed there is reason to believe that the former is not only a disease of the nervous system, *as I pointed out several years ago*, but is in no essential respect different from the latter. A case occurring in my own experience, and reported in the essay referred to (Chlorosis a Disease of the Nervous System.—*Quarterly Journal of Psychological Medicine*, July, 1868, p. 417), is so apposite in the present connection that I quote it in full." (Case referred to later.)

One portion of this paragraph contains a claim to originality so unmistakable that it had better be disposed of at once. In July, 1868, Dr. Hammond "points out that *chlorosis is a disease* of the nervous system," but long years before he had a name the Great Laennec advanced the same view, and Trousseau, at least twenty years in advance of Dr. Hammond, insisted on Laennec's idea in no very equivocal terms. Can Dr. Hammond claim ignorance of the opinions of so eminent and widely-known a lecturer on medicine as Trousseau, the *third edition* of whose classic lectures was published in 1868? Strange admission, indeed! but stranger still, if, acquainted with them, he should publish the foregoing to the world!

The very heading of Trousseau's lecture on *chlorosis*, which indeed is highly instructive and significant also in relation to this *case* of Dr. Hammond's, speaks for itself. It is "On false chlorosis or tuberculous anæmia—Preparations of iron should not be prescribed in this false chlorosis—Tuberculous chlorosis should be treated by bitters and arsenic—On false chlorosis or syphilitic anæmia—Action of the vaso-motor nerves in producing the vascular murmur—*True chlorosis is a nervous disease; the alteration of the blood is secondary.*"

Thus an author, known throughout the civilized world as one of the few great lecturers on practical medicine, has been lecturing and publishing for a quarter of a century or more the opinion that true chlorosis is a disease of the nervous system—not "cerebral hyperæmia," however—and then Dr. Hammond blandly informs us *that he pointed out this same fact, after the third edition* of Trousseau's *lectures* was given to the world.

Here is an instance of gross ignorance of current medical literature on the part of the author, or an assumption of such existing among his readers.

To return to the "case"—

"A. G., a young lady aged eighteen, and in good health, was frightened by a runaway horse which, dashing furiously down the road along which she was walking, scarcely allowed her time to spring

aside, and escape injury. She gets home, and after taking a glass or two of wine recovers her usual good health and equanimity. Her previous health was unexceptional, but soon after the occurrence of the event referred to, her friends noticed the beginning of a gradual change in this respect, but especially in her susceptibility to nervous impressions. From having been cheerful and buoyant in mind, she became melancholy and depressed, etc. At the same time her temper grew irritable and peevish. The slightest sudden noise caused her to start; a bright light disturbed her; a slight touch was magnified into a severe blow. Unpleasant odors were perceived and taste perverted. Her appetite was capricious and irregular, and dyspepsia with constipation supervened. The heart palpitated violently on slight exertion, and the cervical and dorsal regions of the spine were tender on pressure. All these changes were effected within a month after the occurrence of the shock to her nervous system already mentioned. During this period I repeatedly *examined the blood microscopically, and always found the normal proportion of red corpuscles to be present. Finding no change in the blood, and detecting no bellows murmur at the base of the heart or along the course of the great vessels, I did not suppose the affection to be chlorosis*, for I had been taught to *regard this disease* as due to anæmia, and as always being as-

sociated with the signs and symptoms of that condition. From the history of the case, and the prominent phenomena of its course, I was disposed at first to regard it as one of those hysterical affections so frequently met with in young women, and which in some respects it actually resembled. I, therefore, treated it for the first two or three weeks with antispasmodics and tonics, the latter consisting of *quinine* and *iron*, and recommended good nourishing diet and moderate physical exercise. *Had the existing condition been one of anæmia these measures would have been clearly indicated, and would doubtless have proved beneficial.*"

We are then entertained with further details of the treatment, together with a complete analysis of the blood (which is perfectly normal); the unpardonable bungling of another practitioner, and finally we are informed that *strychnia* and *arsenic* are the *source of her salvation.*

"I gave her ten drops of Fowler's solution, and the thirtieth of a grain of strychnia three times a day. I decided on this treatment for the reason that the disease appeared to me one in which derangement of the nervous system formed the most prominent feature, and because I had previously in other affections satisfied myself of the *efficacy of arsenic and strychnia in giving tone to the exhausted nervous centres, and in relieving those functional de-*

rangements of the viscera due to disturbed nervous action."

I do not wish to detain the reader further with quotations from this remarkable case, but if the author does not stultify himself by the record of it, then tolerably plain English is not intelligible to me. First of all, it is given to us as an illustrative case of *chlorosis*. Chlorosis without the only two features that distinguish it from other anæmias!! *No change in the blood and no bellows murmur!* Wherein then is the evidence of *true chlorosis?* Again we are informed, of course, that it is all the result of "cerebral hyperæmia," and behold, it is cured by *arsenic* and *strychnia*, which the author himself has found to give *tone to exhausted nervous centres!* Here, surely, is another instance of "bankruptcy" of the perceptive faculties, for we find in pages 83 and 84—in the chapter on "Morbid Anatomy" by the way—that the indications of the existence of this direful new disease are: 1st, the redness of the face, and the throbbing of the cephalic arteries indicate an increased determination of blood toward the head . . . . 7th, the effect of *quinine*, *strychnia*, alcoholic liquors and other agents, which *we know increase the amount of blood in the brain in aggravating the symptoms of the disease!*

The young lady, it will be observed, had all three

agents denounced in "canon" 7. It may then be unhesitatingly asserted that the author himself establishes beyond a reasonable doubt: 1st, that his illustrative case of *chlorosis* as the result of "cerebral hyperæmia" is not true chlorosis at all; and 2d, that it could have had no connection whatever with his "impossible" disease, because it lacks the two distinctive features of true chlorosis, and is cured by the very means which he himself plainly tells us "only" tend to aggravate the symptoms of "cerebral hyperæmia." It is scarcely necessary to pursue this matter further; but lest the claims of the author, that he was the first to point out that true *chlorosis* is a nervous disease—and, further, lest his assertion that it is a consequence of "cerebral hyperæmia" should in any degree be accepted through the amount of mere verbosity brought to bear on the question, let us compare a few extracts from the lecture of Trousseau on this subject.

After having given an illustration of syphilitic anæmia unaffected by iron preparations, but restored by mercury and iodide of potassium, he proceeds:

"I have persisted in bringing these facts under your notice,* in order that you may thoroughly understand what a multitude of causes can change the composition of the blood so as to resemble chloro-

* This hasty translation has a claim to nothing more than a true representation of the author's views.

sis, and I have done so, above all, to put you on your guard against the evil effects of iron preparation, insufficient in the majority of cases, useless in some, and frequently dangerous in others."

Discussing the causation of the "bruit de souffle" he says—

"There is then no doubt that in *true chlorosis the vaso-motor nervous system is changed in some special manner*, and that this change is to a certain *extent independent of the composition of the blood.* It does not correspond with the increase or diminution of the red globules, since those suffering from anæmia rarely have the double vascular murmur, and again the sufferers from *chlorosis* continue to present this symptom even long after the composition of the blood is restored.

"What I have just said, gentlemen, is sufficient to give you to understand that, in my opinion, *chlorosis must be ranked in the category of nervous diseases.* Laying aside for a while the constituent elements of the blood, let us observe by what phenomena, other than the pallor of the tissues, this disease is manifested. These symptoms have almost exclusive reference to the nervous system. The intellect, sensation, and the motor power of the muscular system, both voluntary and involuntary, are profoundly affected. It is rare that a young chlorotic girl does not suffer from those perversions of the understand-

ing of which so many examples have come under our notice. She becomes irritable and frivolous; and the affection of the mind sometimes progresses to insanity. If we examine the sensation power of the skin with caution, we shall find anæsthesia in a large number of points, and again we find hyperæsthesia over a smaller number. Whenever in your presence I have examined *chlorotic* women I have invariably questioned them as to any neuralgic pains that afflicted them, and you have been able to see for yourselves how uncommon it is to find one who has not suffered from neuralgia more or less violent.

"Spasmodic affections of the voluntary muscular system are quite frequent, and you know how common hysterical convulsions are among chlorotic women. But palpitation of the heart, and spasms of the stomach of the intestines, and of the uterus, nearly all those afflicted with chlorosis suffer from.

"This disturbance of the nervous system does, however, bring about a radical change in the different secretions of the economy. The gastric juices are changed in their chemical composition, and hence the water brash, and eructations, flatulence, etc. The secretions of the liver and kidneys at one time suppressed, at another greatly increased, bear sufficient testimony to the nervous disturbance on which I have frequently dwelt; and the great secre-

tion of the ovary, which performs one of the most important functions, is quite frequently suppressed, together with the menstruation, which is its consequence.

"It is unnecessary to remind you of the many recent experiments conducted by the ablest physiologists, which go to prove what powerful influences nerve disturbances exercise over both the secretions and the component parts of the blood. We can readily understand that, when the function of a blood-making organ like the lung, liver or spleen, is radically changed, the components of the blood itself must be vastly altered.

"This *influence of the nervous system will betimes manifest itself with singular rapidity*. You remember a young woman who had already twice entered our hospital service to undergo treatment for chorea. She had excellent health until badly frightened one night. From the following day her health commenced to fail, and four days after she came to our ward with all the symptoms of a confirmed chlorosis. And in No. 3 of the same ward, we have a young woman of eighteen years old, who, in like manner, after some violent emotion, became chlorotic in a few days. This *indicates to you how little importance we must primarily attach to the condition of the blood*, and *how very necessary it is, on the contrary, to give anæmia a mere secondary place*."

What more intelligible or forcible language could any writer use to insist on the proposition, that chlorosis is primarily a nervous disease, and that the condition of the blood is a mere sequence, than is herein expressed? And yet Dr. Hammond, with characteristic modesty, informs us that he "first pointed out this fact to the profession in 1868." What further proof need be adduced of his utter unreliability as a medical authority; for, assuredly, if the old logical aphorism—*Falsum in uno, falsum in omnibus*—has any force in ordinary life, it must apply with much more vigor to the ordinary practice of medicine wherein so much deference is paid to prominent authorship? The unveracity of the medical writer is not unfortunately limited to his own sphere or generation.

Chlorosis, then, being accepted as a disease of the nervous system, the next question arises, Is it the result of a disturbed vaso-motor system, as Trousseau claims, or is it dependent on "brain hyperæmia," as Dr. Hammond asserts? If we first had any proof of the existence of "cerebral hyperæmia," and after this some connection traced between this hyperæmia and true chlorosis, the subject may admit of some discussion, but, none such existing, it is scarcely a matter to waste the reader's time over. On the other hand, Trousseau, with customary lucidity, goes directly to the base of the subject when he says—

"I need not here remind you of those recent experiments prosecuted by the most expert physiologists which demonstrate the influence that the various disturbances of the nerve functions exercise at the same time over the formation and composition of the blood. We can readily understand that when the special functions of the blood-producing organs —such as the lungs, liver and spleen, etc.—are altered, the composition of the blood itself must also undergo considerable change.

This is at once a philosophic and readily comprehensible view of the cause of true chlorosis, but to trace it to hyperæmia certainly requires some proof further than the mere assertion of one whom we find so constantly adopting views, or advancing them, which, almost in the same page, he again, wittingly or unwittingly, condemns.

The following, taken from this chapter on "differential diagnosis," is by no means a solitary instance of what I have just said—"Dr. Hadfield Jones has, under the name of 'hyperæmia of the brain,' described a condition bearing but slight resemblance to that under consideration, but rather in its main features assimilating with congestion; but the disorder which he designates 'cerebral excitement' is so very like what I would term hyperæmia, *that I am quite sure we refer to the same pathological entity. Indeed, he expresses the opinion that the arterial*

vessels of the brain are in a state of repletion, but he regards this condition as secondary to another which he believes to be an excitation of the tissue. The cases cited by Dr. Jones all present more or less affinity with 'cerebral hyperæmia,' as I have described it in the present essay. I quote one which was under his own care." (Dr. Hammond.)

"H. T., aged fifty-two, was admitted November 25, 1866. A very strong-made, large, hardy-looking man, who has achieved well-deserved repute by his able and successful management of a well-known life-boat. I saw him first October 29th, when I made the following notes. About six years ago, he got a fright when he was out with the life-boat, from a man being washed overboard. He felt 'his inside run round' and he became giddy, but did not lose consciousness, and went on with his work. He never got quite right after that night; his head has been affected ever since. At present he is quite unnerved; *gets no sleep at night*,* being troubled with *dreaming and fancies*; in fact has a degree of delirium—does not know what he is about. Has much sweating at night, and is either 'all on a work,' as his wife describes it, with his arms and legs, or else he is busy electioneering or cutting arms and legs, or singing, etc. The tip of his tongue gets very

* This deserves special notice in view of the treatment adopted and Dr. Hammond's *notions relative to sleep and anæmia*.

sore, too, at night. Every morning for years since he has been ailing, he has vomiting and purging on getting up; it does not occur during the day if he keeps quiet. Is so irritable, if he worries himself at all, he gets all in a tremble. Is often obliged to come home, and go to bed two or three times a day. His limbs are full of aches and pains in blowy weather. His memory fails very much; no paralysis; pupils normal; no strabismus. Head not unduly warm (probably Lombard's instrument would have told a different story*); is *not anæmic.* Manner quiet; has always been temperate. Appetite bad; tongue natural. Is worse than he was a year ago. At the time of admission he was rather better than he was a month before, since he had kept quite still and done no work. His urine was of sp. gr. 1020, of full red color; not albuminous. He was ordered *strychnia, nitric acid, valerian, morphia, hyoscyamus;* and for diet, cocoa, one *pint of ale and four ounces of brandy.* The dose of *strychnia was increased,* the valerian was replaced by *quinine,* and this again by citrate of iron and quinine. By January 30th, he had recovered so far, that he had lost almost all trace of nervous disorder, and returned home well and hearty though by no means capable of resuming his former arduous exertions." (Hadfield Jones.)

* One of Dr. Hammond's modest assumptions that he has a monopoly of the mode of ascertaining temperature.

Such is Dr. Jones' case, and Dr. Hammond immediately adds—"I should certainly call this a case of 'cerebral hyperæmia' as I understand it."

In the name of all common sense, on what grounds?

Does the gentleman already forget the celebrated Canon No. 7, in his marvellous chapter on "morbid anatomy" which I take the liberty of repeating —"The effect of *quinine, strychnia, alcoholic liquors* and other agents which we *know to increase the amount of blood in the brain in aggravating the symptoms of the disease*"?

Thus an eminent observer describes a condition that he is unable to find any definite pathology to explain, and calls it very properly "cerebral excitement," which it will be remembered Dr. Hammond is quite sure corresponds exactly with what he calls "cerebral hyperæmia." Further the "cerebral excitement" is treated successfully with the remedial agents that Dr. Hammond assures us can only tend to aggravate "cerebral hyperæmia"; and yet in face of these facts we have the strangely painful spectacle of Dr. Hammond disagreeing with Hadfield Jones, and insisting on claiming that this condition is nothing more or less than "cerebral hyperæmia."

Unreason can scarcely reach more absurd limits, and it is certainly a deplorable condition of public

and medical education that a mind which has evinced only this capacity throughout, should be regarded as anything above the merest irresponsibility. To regard one as an authority on the very highest branch of medical science whose efforts have nowhere shown a higher range than that pointed out here, is a sad commentary on what might be called the medico-social condition of our profession throughout the country, because, if a proper *esprit de corps* existed, the folly of such aspirations would readily be exposed. At every page the same reckless disregard of consistency confronts us, and it seems almost incredible that such utter obliviousness of what one paragraph contains, should be implied by the contents of another immediately succeeding. For example, we are cautioned against confounding anæmia with hyperæmia, as the consequences may be disastrous, and then informed that "the condition often induced by *shock presents some features of resemblance to cerebral hyperæmia:*" Thus there may be loss of consciousness, irregular action of the heart and pain in the head. But the *pallor* of the *face*, the dilatation of the pupils, the coldness of the skin, and the feeble respiration and pulse, are sufficient for the ready recognition of the real character of the state in question.

"In a somewhat less severe form of cerebral *anæmia* there may be *illusions and hallucinations and*

some confusion of ideas. A low form of delirium may be also present. But again there are the dilated pupil,* the feeble heart, the pallid face, the cold skin, so characteristic of deficient blood-supply to the brain."

In this manner the writer seeks to distinguish between *shock* and *hyperæmia*—a matter that should be judiciously attended to. But will the reader kindly turn to his case—his novel case of chlorosis already quoted, where he will observe that "hyperæmia cerebral" and *chlorosis* are only different names for the same condition. In that case he will surely find *pallor of the face* and *illusions* and *hallucinations*, and, moreover, he will also learn from the author himself that the whole disturbance arose from shock—shock, and nothing more! In unmistakable language the case of chlorosis recorded is stated to be the result of "cerebral hyperæmia," which in turn is directly attributed to *shock*. And now we are strenuously cautioned against confounding *hyperæmia* with *shock* as the result would be lamentable! Another instance of brain bankruptcy, no doubt. Amid the gloom, however, it is pleasant to remember that all

* That "dilated pupil" should be regarded by Dr. Hammond as indicative of cerebral *anæmia*, requires some explanation, considering that in his view *sleep* is the *result of cerebral anæmia*, and in this condition the most casual observation would have shown him the pupil to be in a state of contraction.

the treatment in *the* case of "chlorosis" was anything but of a nature to subdue "cerebral hyperæmia," and hence that the patient—whilst her disease may have been the theme of an untenable nomenclature—has at the same time the benefit of a sound and rational medication.

In this "differential diagnosis" chapter we now come to one of the most important—if not the most important, and at the same time most vicious, portion—of all Dr. Hammond's labors—the one which he claims most especially his own, and regarding which there is a stout claim to originality* pervading each of his numerous publications. It is this: "That sleep is due to diminished blood-supply to the brain."

"And to supplement all this we have to recollect that in "cerebral anæmia" vertigo is scarcely ever a prominent feature, and that, *so far from being wakeful, the patient* is, on the contrary, *almost constantly drowsy*. No diagnostic mark is of so great importance as this latter, and I regard it as of *itself sufficient to determine the question. Sleep is the result of a diminished amount of blood in the cerebral vessels*, and when this diminution does not pass a certain normal limit, natural sleep results. If, however, the

* The claim to originality herein will be found to be as defective as that of having been the first to "point out that chlorosis is a nerve affection."

brain is to an inordinate extent deprived of its circulating pabulum drowsiness is the consequence; wakefulness, on the contrary, is produced by an excessive amount of arterial blood in the brain, and hence it becomes a prominent feature of cerebral hyperæmia. It is true that in the advanced stage, when instead of hyperæmia there is rather congestion, *stupor* may ensue, but *stupor* is by no means *sleep*."

This *invaluable* passage may be said to contain four important announcements—1st, that stupor is by no means sleep; 2d, that cerebral anæmia is almost always accompanied by drowsiness; 3d, that this fact of itself is sufficient, in the author's estimation, to distinguish anæmia from hyperæmia, which is invariably attended with wakefulness; 4th, that sleep is the result of diminished blood supply to the brain.

I am forbidden by the proposed limits of this essay to enter on as complete a discussion of these propositions as their nature may demand, but I trust that a mere hint will enable the interested reader to follow up the subject for himself, and I feel assured that the more diligently he pursues the investigation, the more baseless will appear the latter three propositions in this category, and the more injurious in treatment if carried to their logical results.

With regard to the first proposition, there can be little question if we confine ourselves to the *stupor* or coma incidental to apoplexy, uræmia or typhoid fever as distinguished from healthy natural *sleep*, but then the *stupor* of opium or alcohol (which unquestionably increase* the blood supply in the brain) is rather difficult to distinguish from the *sleep* of the bromides, which the author himself assures us is the direct result of a diminished blood supply. When the same apparent result (to our untutored observation) is produced by agents claimed to have such diverse physiological action, why it should be called *stupor* on the one hand and *sleep* on the other, requires some satisfactory reason, but as in so many other things in this also we receive none. The fallacy of announcements two and three can readily be inferred, if it be shown that the fourth is without foundation. To prove this, it is only necessary to refer to the author himself. We find in the case previously quoted from Hadfield Jones, and passed to the credit of cerebral hyperæmia by Dr. Hammond, that such a list of circulation stimulants, as, *morphia*, ale, brandy, *strychnia* and *valerian* produced total recovery of a patient who "gets no sleep at night" until placed under this plan of

* See standard authorities in therapeutics *passim* as well as writers on medical jurisprudence. See author himself, Canon 7. in "Morbid Anatomy."

treatment. In one of his introductory remarks also the author tells us a very well recognized fact, that *intellectual* labor produces cerebral hyperæmia, and is it not a natural converse of this proposition, and quite in accord with what is elsewhere observed in the economy, that intellectual or brain *repose* or *sleep* should produce more or less anæmia? But despite this proposition, one part of which he recognizes, he persistently asserts that it is the anæmia produces sleep. That his view must be incorrect is further proven by the effects of most narcotics, such as chloroform, opiates, chloral and alcohol as stated above—substances which all therapeutists claim to increase the quantity of blood in the brain—but which we have to call into constant requisition for the purpose of producing sleep.

Dr. Hammond, it is true, claims to have shown by practical experiment that the bromides produce sleep, by lowering the brain circulation. From a comparison of the action of many substances that are constantly used to induce sleep, it is not too much to claim that his conclusions must be erroneous; but on the contrary that the bromides giving rise to *sleep*—not in the manner claimed—the sleep then in turn—no matter how induced—tends to lower *pro tanto* the brain circulation. Epilepsy is conceded by pathologists, to be a disease accompanied by brain anæmia, *i.e.*, prior to the spasm, and the treat-

ment recommended by the best authorities is of a nature to improve the blood in quality and quantity generally—yet we know that no one drug has such a powerful influence in its subjugation as one or other of the bromides—an additional proof that their tendency cannot be (as Dr. Hammond concludes) to produce directly brain anæmia, else assuredly epilepsy, far from being benefited, would necessarily be aggravated by them.

But, lest our common observations and the author's own inconsistency be insufficient to refute this great claim of his, let us again glance at the most recent and able compendium of physiology (Foster, pp. 717).

After having shown that all portions of the body participate in sleep as much as the cerebral hemispheres he proceeds: "We are not in a position at present to trace out the events which culminate in this inactivity of the cerebral structures. It has been urged that during sleep the brain is anæmic*; *but even if this anæmia is a constant accompaniment of sleep, it must, like the vascular condition of a gland or any other organ, be regarded as an effect, or at least as a subsidiary event, rather than as a primary cause.* The explanation of the condition is rather to be

* Durham Guy's Hospital Reports, vol. iv., 1869—(which, it is well to remember, was several years prior to Dr. Hammond's ORIGINAL experiments made by "Mr. Durham and myself.")

sought in purely molecular changes, and the analogy between the systole and diastole of the heart, and the waking and sleeping brain, may be profitably pushed to a very considerable extent. The sleeping brain in many respects resembles a quiescent but still living ventricle. Both are, as far as outward manifestations are concerned, at rest, but both may be awakened to activity by an adequately powerful stimulus. Both, though quiescent are irritable; in both the quiescence will ultimately give place to activity, and in both an appropriate stimulus, applied at the right time, will determine the change from rest to action. Just as a single prick will, under certain circumstances, awake a ventricle, which for some seconds, has been motionless, into a rhythmic activity of many beats, so a loud noise will start a man from sleep into a long day's wakefulness. And just as in the heart the cardiac irritability is lowest at the beginning of the diastole, and increases onwards till a beat bursts out, so is sleep deepest at its commencement after the day's labor; thence onward slighter and slighter stimuli are needed to wake the sleeper.

We cannot at present make any definite statement concerning the nature of the molecular changes which determine this rhythmic rise and fall of cerebral irritability. Preyer's leaning toward this view, that the accumulation of the products of protoplasmic

activity may become in the end an obstruction to that activity, has been led to think that the presence of lactic acid, one of the products certainly of muscular, and probably nervous metabolism tends, to produce sleep; *but this is doubtful.* The suggestion of Pflugen that the diminution of irritability, and consequent suspension of automatism, is dependent on the exhaustion of the store of intra-molecular oxygen (p. 364) is more worthy of attention.

The phenomena of sleep show very clearly to how large an extent an apparent automatism is the ultimate outcome of the effects of antecedent stimulation. When we wish to go to sleep, we withdraw our automatic brain as much as possible from the influence of all extrinsic stimuli; and an interesting case is recorded of a lad, whose connection with the external world was, from a complicated anæsthesia, limited to that afforded by a single eye and a single ear, and who could be sent to sleep at will* by closing the eye and stopping the ear.

The force of these remarks needs no comment; all therapeutical and clinical observations corroborate them, and until we are educated by a series of new experimental facts, we must continue to accept

* Possibly Dr. Hammond may explain how this closure of the eye or stoppage of the ear produced the amount of brain anæmia necessary for sleep in his calculations.

this view of sleep though in complete antagonism to the assertions repeated by Dr. Hammond throughout his numerous writings, and as constantly refuted by his more rational methods of treatment.

Not far from this paragraph regarding *sleep* and its mode of induction is another no less strange, if less important. It is an effort at general instruction to distinguish between hyperæmia and "ramollissement cerebri," and runs: "In fact, the two have scarcely any but the most superficial resemblance. In softening, the intellect is seriously and *permanently impaired*, and the *morbid process advances sometimes slowly*, but *almost* always certainly to *a fatal termination.*" With the correctness of this view relative to cerebral softening few will feel inclined to differ, but it will be found rather startling in its confliction with the statements elsewhere made by the writer, and especially in his article on "cerebral softening" (in his larger work on nervous diseases), where so many cases are reported as "permanently cured" and "now quite well,"—some indeed completely restored *after ten days' treatment.**

It would evidently be a loss of time to follow the writer in his distinction between this *new* malady and Bright's or Meniere's disease. Each one of these

* See author's work on "Diseases of the Nervous System," art. "Cerebral Softening," or case recorded in the introductory chapter to this essay

latter has of course its distinctive features which the merest tyro in medicine cannot fail to recognize when the attention is so directed. And considering accordingly how little enlightenment there is in this chapter on "differential diagnosis," what a total absence there is in it of anything really indicating the existence of "cerebral hyperæmia," it is not very astonishing to find this termination to it:

"Indeed, there are few diseases so distinctly marked by their symptoms and clinical history as that which forms the subject of this essay."

The ridiculous *nonchalance* of this *finale* is beyond explanation, unless the writer seeks to defy all professional opinion; or has lost the most commonplace capacity for rational inference.

CHAPTER IV.

GENERAL CONCLUSIONS.

THE chapter on Etiology or causation (of an impossible disease), though containing much of a general nature that is true, but nothing original, is by no means free from the more pronounced defects of its predecessors, as will at once be apparent in the opening paragraph.

"In the introductory chapter, I have considered to some extent the most prolific causes of cerebral hyperæmia, those of a mental or emotional character—and of this category the emotional are by far the more frequent. A sudden SHOCK of this description may, as we have seen, induce the affection instantaneously; but still more common are those instances in which some powerful emotion, acting, day by day, and even night by night, slowly perhaps but with terrible certainty, leads to the more or less permanent increase of the *intra-cranial* arterial blood with all the inseparable phenomena."

It would be quite a work of supererogation to ad-

duce further arguments against the assertion that any cause can produce increase of the *intra-cranial arterial blood*, without a corresponding increase of the *intra-cranial* venous blood, which reduces the matter at once to one form or other of congestion which we all recognize. This matter has been sufficiently gone over elsewhere, and it is scarcely necessary to repeat the conclusion, that even from ordinary hydraulic principles the theory must be untenable. But it is not a little odd to see our old friend "*shock*" coming here into such bold relief again. At one time it was the direct cause, according to our author, of chlorosis or "cerebral hyperæmia" (for we were assured they meant the same thing); at another place we were stoutly cautioned not to confound *shock* with "cerebral hyperæmia," as the *consequences* may be *appalling;* and now again we are blandly informed that a "*sudden shock* may induce this affection instantaneously." This repeated self-refutation will of course be apparent throughout the monograph; but it may be asked, What useful purpose can a further exposition of it serve here? This: that if left unchallenged some practitioner may be inclined to credit the theory, that *shock* led to hyperæmia (cerebral); that, indeed, the brain is the organ exclusively suffering in this condition, and consequently the only one to be treated, which could not lead to other than disastrous conse-

quences. Now, it is needless for me to observe that *shock, i. e.*, moral shock as indicated here, and as we recognize it, leads to a chain of troubles very different from any form of cerebral hyperæmia—that, in fact, the vaso-motor system receives the brunt of the attack, and whatever brain hyperæmia or anæmia supervenes differs in no essential particular, as regards its causation, from the *faintness, nausea, diarrhœa* or *jaundice* that so frequently results from this same cause.

There is another idea of paramount importance pervading this chapter on "causation," which, though advanced with some caution, nevertheless becomes apparent throughout, viz., that intellectual labor predisposes to brain disease. The writer, it is true, recognizing general physiological laws, tells us that the brain of man, far from being injured, is really strengthened by full mental labor as surely as the muscles are invigorated by physical exercise. But then he readily loses sight of this general principle, and diverges off into details of the small amount of mental toil endurable, and adds, that few can safely indulge even in his moderate limit, that the consequence of effort in this direction would be brain-destruction to the many, because, forsooth, "the walls of their intra-cranial vessels are weak, and hence, when once they have become distended, they do not readily return to their normal size."

There can be little doubt of the general correctness of the analogy touching the effects of their appropriate exercise on muscular and nervous tissues; but we now learn for the first time that brain exercise produces *weakness* of the *intra-cranial* vessels, while we know that muscular exercise—judicious, of course—will produce strength and vigor of the vessels pervading muscular tissue. If we are to rely on the author's assertions, however, it is among the class who might be most capable of physical exercise that this weakness of the intra-cranial vessels is most likely to occur; and as this opinion is so directly opposed to all analogy elsewhere, to all the recognized laws of physiology, it would be only simple justice to inform us how the intra-cranial vessels are affected so differently from the other vessels of the system, by the same causes. But all forms of demonstration are completely ignored in this unique work on Cerebral Hyperæmia, and it cannot be other than unfortunate that assertions so novel and startling should thus proceed, without regard to fact, from so prominent an authority, as doubtless many readers, incapable of judging for themselves, may in consequence be deterred from beneficial and vitalizing mental effort. There is now no question of the fact, that judicious physical exertion will strengthen the muscles and blood-vessels generally, and that what we recognize

as *fatigue* is the one indication for a discontinuance of exercise: that this indication of *fatigue* is further and further deferred as the parts, by effort, acquire increased vigor.* Nor does it require any but the most commonplace observation of every individual to discover that exactly the same rule holds good in the case of mental exercise; that the untrained brain readily tires of intellectual toil, that this fatigue demands rest, and that the sense of fatigue is further and further deferred, as well in purely mental labor as in the complex labor of the musician's hand, as progress is made in intellectual strength by judicious mental toil. Dr. Hammond evidently would have his readers confine their brain labors to very narrow limits, which, if followed implicitly, should have deprived us of many a great and noble work of the past, and may deprive us of still greater achievements in the future. The fact is well recognized that perseverance, persistent effort by comparatively dull brains, *i. e.*, those at first easily fatigued, has done more for the world tenfold than the spasmodic efforts of the bright and gifted, to whom mental effort was never fatiguing, and if the former had been taught to dread a giving way of their intra-cranial vessels, how few of the achievements that mankind are proudest of would

* It is very questionable if the nervous system does not play a leading role, even in what is regarded as solely muscular exercise.

be transmitted to us! If his doctrine had any force at all, one of the first results should naturally be, that a larger percentage of mental alienation, brain bankruptcy, or whatever he may choose to name it, would be found among the members of the learned professions and literary men than among any other class. Statistics on this subject, if ever published, are not at hand, but I feel confident that such would be very far from aiding his theory. The *unfortunate habits* of the American public, whose brains (we are assured) are so much more strained than those of other nations—a hackneyed field for meaningless platitudes—should, according to our author, lead to a very much higher percentage of insanity among them than is to be found among other nations. Such is not the fact, however; but on the contrary, as far as ascertained, the proportion of insane to population is a little less in these States than in the civilized nations of Europe.

There can be scarcely a question among physiologists and practical physicians at the present time, that not only the mind—or brain, its organ—as a whole can be educated by effort, by labor, but that also the different faculties of the mind—different portions of the brain no doubt representing them—(and this subject is worthy the attention of those small philosophers who scoff at moral training) can be cultivated beyond limits previously unthought

of, just as the muscular system of individuals—pedestrians, for example—has acquired a capacity for endurance heretofore considered impossible.

There can be little doubt that Reason, Memory, Imagination, and even the Will itself, can in almost any individual be brought to a higher standard by effort than he may himself at first regard as possible, and in this way, as we so often observe, the moral discipline is acquired by which one is able to abandon the vice which has swayed him for years. All this form of training might be frequently avoided if the individual should accept the theory that by any or all forms of mental effort the walls of his intra-cranial vessels would be likely to succumb, and no doubt much of the hypochondriasis among intelligent persons, who come before us so often, arises either directly or indirectly from the promulgation of doctrines of this quality. The fact is that mental toil—not the enfeebling pursuit of shadows—is as necessary for the brain's continuance in a healthy condition as proper muscular exercise is for the preservation of physical vigor, and there is quite as little danger to the intra-cranial vessels from the one, as there is to vessels that pervade the muscular structure in the other. From what has just been said, however, it must not be inferred that I regard it as anything like impossible to injure the human brain by over-study. On the contrary, as

fully as Dr. Hammond, I recognize that injury may result, but not from the same amount of cause, or in the same manner. For instanee, the child's muscular system is easily over-strained and injured, and in like manner the child whose brain is over-taxed may be perpetually injured. Furthermore, the child who is forced into an uncongenial groove, whose mind is perpetually crammed with subjects for which there is no receptivity, as it were, is very apt to have faculties of a more powerful, more useful nature, dwarfed into insignificance by the very effort to force another, and thus a mind that may become conspicuous in some useful sphere is stunted in all by the injudicious effort of parent or teacher, in striving to force it into a beaten track.* Again the imposition of too much study on the youth—study even of the most congenial nature—is a source of danger, for the exhausting brain-labor, as well as the sedentary habits necessary therefor, will lead to general constitutional debility, phthisis, etc., in which, of course, the brain itself soon participates.

These views are common to the ablest physiologists and physicians throughout the world, and few modern thinkers in this sphere will agree with the formulated rules of Dr. Hammond relative to eight or ten hours' study in general, and that "mathe-

* This, of course, would also apply to older persons in a minor degree, from the necessity of making a suitable livelihood, &c.

matics are specially injurious to ladies." This subject admits of no rules, and the old adage, "*Chaque un* a son gout" should be under proper early training the guide in all cases no less in the female sex than in the male. The general principle that is sought to be established, and one with which few observant men will differ, is this: that the hours of study of any healthy, congenial nature can be increased to very extended limits, by effort and training, without injury to the nervous system, just as muscular exercise can be vastly increased by the same means, and that the mental toil which at first readily expresses itself by fatigue, just as in the case of the physical system, will gradually become less and less tiresome, as the mind becomes more inured to the form of labor to be undergone.

There can be no necessity of prolonging this discussion on the causation of this factitious malady, but the general recklessness of treatment is well shown by Dr. Hammond when he produces an array of "causes" for the support of his position, which Dr. Richardson has already adduced as giving rise to totally different consequences, and then makes lengthened quotations from Dr. Richardson's work on "Physical Diseases from Mental Strain," while this writer nowhere recognizes a disease arising from "Enlargement of the intra-cranial capillary arteries."

The following also among the "causes" cannot

fail to prove interesting: "There are many substances which, when taken into the stomach or inhaled into the lungs, increase the amount of intracranial arterial blood. Among these are the various alcoholic liquors, camphor, opium in small doses, belladonna, Indian hemp, quinine, sulphuric ether, chloroform, nitrate of amyl, &c. A like power is also exerted by tea and coffee, and probably in some cases by tobacco."*

When we are told that these substances produce merely an increase of arterial blood, as our standard authorities inform us that general blood stasis arises from their use, in consequence of their capacity to prevent natural metabolism, it would plainly be the duty of the writer to inform us as to how he has arrived at his conclusion. The one expression in this paragraph, of "opium in small doses" is, however, a slight diplomatic concession to our preconceived and pretty well established views relative to the action of this drug, inasmuch as the only doses —poisonous ones of which we can form an absolute definite opinion—act in a somewhat different man-

* In view of what has been uttered in previous chapters, there is nothing very singular in what we notice here. A disease of the cerebral arteries, lasting over a year, is ascribed to drugs so transient in their action as alcoholic liquors, morphia, chloroform, sulphuric ether, &c. While we observe elsewhere that these same drugs are commonly prescribed to *cure* the disease of which they are here regarded as the cause.

ner. According to all observers, therapeutists, and medical jurists, poisonous doses of opium produce immense enlargement of the intra-cranial venous system, or cerebral congestion. But how it can in smaller ones produce enlargement of intra-cranial arteries merely, is a problem that awaits some new therapeutical and physiological enunciations which, from the monograph before us, is not at all beyond the reach of our author's astounding capacity.

The chapter devoted to "Prognosis" has one decided merit—brevity—and is futher consoling in the fact that the writer has never "*seen* a *patient* die of simple *cerebral hyperæmia*" (a fact which the reader may have already inferred from the impossibility of its existence). The reader, however, who can fathom the meaning of the following has a capacity for discernment greatly to be *envied:* "The tendency of the affection, if it be not arrested, is to pass into one or other of the fully developed forms of *cerebral congestion* already mentioned, and then the liability to secondary lesions such as *softening cerebritis, cerebral hemorrhage, aneurism, general paralysis,* &c., must be taken into account. The *apoplectic* form of *cerebral congestion* is that to which there appears to be the greatest predisposition, and the *epileptic* and *paralytic* come next in order. The *soporific,* the *maniacal* and *aphasic,* are more rare, but are occasionally met with."

This jumble of mere grandiose verbiage is sufficient to "stop the breath" of the thoughtless or mayhap uneducated reader, but to the professional mind it has no meaning whatever. What is the difference, for instance, between *softening cerebritis* and *general paralysis?* None whatever. Synonymous terms merely; and that hyperæmia, even as recognized, has aught to do with the causation of cerebral softening is a doctrine of antique and obsolete pathology. The same is true of apoplexy; and the additional information that there is an *apoplectic* and *epileptic* form of *cerebral hyperæmia requires something like proof*, while the distinction between the *paralytic* and *apoplectic* needs nothing less than *genius* to demonstrate, inasmuch as every appreciable form of apoplexy produces more or less physical or mental paralysis. The *soporific*, *maniacal* and *aphasic* varieties are truly overwhelming! According to its etymology, soporific has undoubtedly something to do with *sleep-producing*, and yet we have been told over and over again that cerebral hyperæmia is essentially a condition of *wakefulness*, while now we are introduced to the *soporific* variety of it.

"*Dormitat magnus Homerus*," verily!

With regard to the *aphasic* variety of cerebral hyperæmia, it is not too much to say that, taking the sentence just quoted as an exposition of his views, he must be sadly ignorant of what aphasia means,

and the discovery of Broca must be to him a veritable "*terra incognita.*"

We now come to what must necessarily be the crowning absurdity of this whole monograph, viz.: the chapter on "Morbid Anatomy and Pathology," an effusion of several pages without a trace of "morbid anatomy" to enliven it (which could not be well otherwise, seeing that the author—or any one else, for that matter—had never known a death to occur from this imaginary disease).

Instead of "morbid anatomy," however, we are treated to the following extraordinary series of "cannons" as an assurance of its presence:

"1st. The *redness* of the face and the throbbing of the cephalic arteries* indicate an increased determination of blood toward the head.

"2d. The *sensation* of *fulness of the head almost invariably present.*

* Discussing *cerebral hemorrhage*, Niemeyer makes some remarks that Dr. Hammond in this case may well take to heart: "In all large hemorrhages this anæmia may not only be recognized with certainty after death, but even during life it shows itself by a very important symptom, which is usually falsely interpreted; that is, by a remarkable pulsation of the carotids. This symptom is very generally regarded as a sign of 'increased pressure of blood to the head,' although it really indicates that the flow of blood into the skull is obstructed; we may at any moment induce the same phenomenon in the artery of the finger by tying a string tightly around the end of the finger.

"3d. The increased heat of the head, not only subjectively, but as indicated by the thermometer, or the thermo-electric calorimeter.

"4th. The *persistent insomnia always present*—a *condition now known to be due to cerebral hyperæmia.**

"5th. The aggravation of all the symptoms of *intellectual exertion* or emotional disturbance.

"6th. The results of ophthalmic and aural examinations.

"7th. The effect of *quinine, strychnine, nitrite of amyl*, alcoholic liquors, and other agents which we know increase the amount of blood in the brain, in aggravating the symptoms of the disease.

"8th. The speedy disappearance of the phenomena under the influence of mental quietude, and of those therapeutic agents which lessen the amount of intra-cranial blood."

The ordinary student of medicine could only place those "cannons" in the category of Symptoms, if anywhere, for assuredly between them and what is commonly regarded as "morbid anatomy" there is no resemblance whatever, and the difficulty of following our author for ten lines consecutively becomes more evident in consequence. I shall not, therefore, ask the reader to follow any detailed re-

* What of the *soporific* variety of "cerebral hyperæmia"?

marks on the assertions contained in them, for their reputation is sealed, not only by the common observation of practical physicians, but by the author himself,* in diverse portions of his work. The task I have undertaken is not to follow all the peculiarities of the writer; it is rather to ascertain how far he is to be relied upon as a guide in general, and hence the two prominent features of all his works, and without those they would have few points of distinction, are the ones that I endeavor to draw special attention to throughout. They are, 1st, that *sleep* is due to cerebral *anæmia;* 2d, that the bromides produce sleep by reducing the blood supply to the brain.

I have elsewhere quoted from Foster what is the intelligible and received view among physiologists regarding sleep at the present time, and which, weighed side by side with the therapeutical effects of the many sleep-producing drugs, has about it a basis of irrefutable argument against which the idle repetitions of Dr. Hammond are unworthy a moment's consideration. To show how frequently these two leading ideas occur in the author's labors it is only necessary to refer even to the chapter on "Morbid Anatomy," where the reader who has been interested by the previous remarks, will find the fol-

* See case of Hadfield Jones commented on ; also chapter on Treatment.

lowing not unwonted platitude: "The principal subjective symptoms of cerebral hyperæmia, when it is of such a degree of intensity as to constitute disease, are wakefulness, pain increased by mental exertion; and *mental aberration* of some kind. Of these the first is the most important* not only on account of the suffering and injury it is to the patient, but because it is the necessary accompaniment, *the* pathognomonic symptom of the affection in question. *Without wakefulness* there is no †*cerebral hyperæmia*, with *cerebral hyperæmia* there is always *wakefulness*." This is a bold reiteration of a theory which never had a shadow of reason to sustain it, and which is as manifestly incorrect as the most absurd of the dreams of the humoral pathologists. It has been so proven long before the publication of this monograph by Dr. Hammond, and hence there is no excuse for its constant recurrence in this work. Furthermore by one who has ever taken the slightest pains to study the physiological action of such substances as chloroform, alcohol and opium—all sleep-producers of the most unmistakable character—this worthless

*Ordinary minds would conclude that mental aberration or insanity is at least as important as mere wakefulness.

†This, it will be observed, is the manner in which "morbid anatomy" is served up throughout the chapter.

repetition of a mischievous, misleading doctrine could never be so persistently reproduced.

We are told that "the experiment of Mr. Durham and myself proved this matter beyond a question," though it is unnecessary to remind the reader that the experiments of Mr. Durham were made *five years prior* to any publication by Dr. Hammond on this subject, and that as regards their value in connection with this subject they have no weight whatever. All that the experiments demonstrated was simply this: When the animal "went to sleep," the brain showed a lessened quantity of blood flowing through, and when the animal awakened the quantity increased. Now do we not observe this same result constantly in the case of the babe whose anterior fontanel is yet unclosed? Even when the child sleeps, as it commonly does, after its natural repast, and when, as we know, the general activity of the circulation, and consequently that through the brain, is decidedly increased, the fontanel will become more or less depressed, a fact which also conforms with our accepted ideas of physiological laws. For it is now indisputable that, *ceteris paribus*, the amount of blood directed to any organ of the body will be in direct proportion to the activity of the organ at the time, so that the quiet of the brain, the repose which the condition known as sleep manifestly produces, is the cause of the lessened blood

supply of the brain, and not the consequence of it, as more clearly elsewhere enunciated by Foster. Nor, indeed, could anything well be more corroborative of the falsity of his position in this regard than the very cases quoted by himself to sustain it. Take the following for example: "Dendy states that there was in 1821 at Montpellier a woman who had lost part of her skull, and the brain and its membranes lay bare; when she was in a deep sleep the brain remains motionless beneath the crest of the cranial bones; when she was dreaming it became somewhat elevated, and when she was awake it was protruding through the fissure of the skull."

The question now arises: What produces the hyperæmia in this and similar conditions, if the dreams be the result of hyperæmia? Sleep is no doubt a continued, unchanged condition as long as it lasts notwithstanding the presence of dreams, and as long as the individual remains undisturbed by any external influence in the same bodily position, there could, in a case like this, have been no external agency producing first hyperæmia and consequent dreams, while on the contrary recognizing the opposite view, that the action of dreaming produces the hyperæmia, we at once arrive at a perfectly satisfactory and philosophic explanation. We know that dreams arise in relation to thoughts that have crossed the *mind with* more or less impress, on prior occasions,

that while more urgent matter, as it were, presses them aside in our wakeful moments, they again and again recur in the somnolent brain and assert themselves like the thoughts we sometimes cannot banish whilst awake. Now, one of these passing thoughts asserting itself in this condition of sleep, will, of course, give rise, according to its importance, to a greater or less degree of mental or brain activity, which activity in turn produces, in conformity with all analogy elsewhere in the body, the observed hyperæmia. Strangely, but consistently enough, the action of hypnotic drugs is also introduced to add proof to his doctrines in this regard, and we are told that: "All those which produce sleep probably do so by their action in lessening the amount of intra-cranial arterial blood."

This subject has been discussed sufficiently elsewhere to show that, so far is this from being the case, that the most powerful stimulant of the circulation, as alcohol, is also a powerful hypnotic, and it is not now too much to add that since the discovery of the circulation by Harvey, no medical teacher, worthy the name, has propounded so crude and mechanical a notion of the action of drugs.

As it stands, it implies this, if anything: that a drug which is absorbed into the general circulation has the power of lessening this same circulation in one direction and increasing it in another, that, in

fact, it has placed in position a series of stop-cocks at certain portions of the system, and can mechanically reduce or increase the flow of blood in certain directions. Possibly at no distant day this philosophic writer will point us out the drugs that increase the circulation of the right arm, and reduce it in the left, and *vice versa*. Clearly the physiology of the vaso-motor nervous system, and its regulating power over the circulation of the different tracts of the body, has not received *all* the attention needed from the author of "Cerebral Hyperæmia."

Again, in the chapter on "Treatment," we find this same mechanical idea of therapeutics emphasized with the customary vigor. We are told that the "bromide of potassium can almost always be used with advantage to *diminish the amount of blood in the brain, and to allay any excitement of the nervous system that may be present in the sthenic form of insomnia.*"

Throughout the paragraph, as in the work generally, this same extraordinary strain prevails, without the slightest apparent regard for fact or previous assertions. If the writer had for a moment considered the action of such drugs as aconite, veratrum viride, and tartar emetic, what conclusion might not have been arrived at relative to their sphere of *usefulness?* These are substances which, it is fair

to presume he will concede, have the direct effect of lowering the general circulation, and consequently that of the brain. Hence, according to his theory, they should be very powerful hypnotics, unless they are defective in stop-cock action; but it is reserved for future investigators to demonstrate that they have the slightest effect in sleep-producing, for, so far, it has not been asserted by any reputable authority, and from our daily clinical observation of their effects, there is anything but evidence of their power to control excitement of the nervous system.

Some *forty years* ago Wilks, of Guy's Hospital, London, discovered that bromide of potassium had a powerful influence in allaying many forms of nervous excitement, and since that time it has been very extensively adopted by the profession for such purposes; yet Dr. Hammond, with customary modesty, claims that he was "the first to announce this fact to the profession *twelve* years ago."

By this time the reader must be somewhat accustomed to this species of assertion, but considering the *ex cathedra* dogma so frequently propounded, to the effect that wakefulness constantly depends on "cerebral hyperæmia," he will be scarcely prepared for the admission above implied, that there is an asthenic as well as a sthenic form of insomnia, for we are carefully reminded that "the bromide of

potassium, by limiting the amount of blood in the brain, will allay any form of nervous excitement dependent on the *sthenic form* of insomnia."

Again, the same happy indifference to consistency as well as to therapeutic discoveries greets us in the next page on treatment, when we are recommended to use, in this incomprehensible malady, the most powerful heart-tonic in the Pharmacopœia, viz., digitalis, for the reason, as we are informed, that "it is of great service in giving strength to the heart." Thus this medical philosopher, who delights in surprises, will instruct the engineer in hydraulics: "Your elastic tubes in this region (and the brain arteries are to some extent closely corresponding), are overdistended; I am very much afraid they will give way; your only proper plan of reducing the strain is to add more force to your pumps"!!!

The foregoing is not, however, the only recommendation in the chapter on "Treatment" worthy of note, not for any information it contains, but as a mere matter of curiosity. The very next paragraph to the above informs us that if dyspeptic symptoms are present we are to use pepsine and charcoal with our meals. Elsewhere in this monograph we were carefully cautioned to guard against the fact that dyspepsia was one of the diseases liable to be confounded with cerebral hyperæmia—a con*fusion* that must be healthily provided against.

Thus Trousseau's idea concerning "vertigo, a stomacho læso," which so closely conforms to our daily observation in practice, herein receives the positive indorsement of Dr. Hammond, viz., that the stomach is primarily the peccant organ in some forms of "cerebral hyperæmia," but unhappily this indorsement is accompanied by a feeble and futile effort at treating the multifarious malady known as dyspepsia. The application of the actual cautery to the nape of the neck is also recommended in treating this factitious disease, but why we cannot understand, unless for a reason corresponding to that given above relative to the use of digitalis, or an unenlightened belief in the old derivative notion of counter-irritants. But disputable points of this character need not detain us, for we will find in the list recommended for *treatment* such powerful heart tonics and nerve tonics as arsenic, strychnia, iron, phosphorus and quinine—all of which it will be remembered were in that terrible "Cannon No. 7" in Morbid Anatomy; and the deleterious effects of which were given as the proofs—indeed, almost the only proof—of the existence of the malady. We find whiskey, brandy, gin and rum strictly forbidden, whilst the light malt beverages, *i. e.*, ales* and beers,

* No doubt these substances are commended for the abolition of "cerebral hyperæmia" because the appearance of brewery men is so characteristic of cerebral anæmia!!

and the lighter wines of France, Germany and Italy strongly recommended, though, as regards the cerebral circulation, what the difference may be between good brandy reduced down to the alcoholic strength of light wine and the light wine itself is a problem that may be worth more to the author to solve than all his contributions to medical literature. Again, tea and coffee may be "left to our own discretion." Coffee! the most powerful stimulant of the circulation in common use! If the reader doubts this let him count his radial pulse immediately before and after having taken a small cup of black coffee. But then, possibly, coffee has the stop-cock action already alluded to, *i. e.*, it may perhaps increase the vigor and frequency of the radial pulse and diminish both in the external carotid artery and so cut off supplies to the brain!!

A light supper before retiring is also commended, and the rationale of its effects are explained: "For food to be digested there must be an increased flow of blood in the stomach, and a part of this comes from the brain to the relief of the surcharged vessels."

This is really profound, and the stop-cock action in physiology is not confined to the action of drugs alone. All physiologists tell us that after eating the general circulation is more active and vigorous, *and* every individual can find this out for himself

by an examination of the pulse immediately before and after any repast. It was reserved for Dr. Hammond, however, to inform the world that while the circulation in general may be more active; that while the radial artery, for instance, may have a quicker and fuller flow of blood through it, the carotids supplying the brain will tell a totally different tale!!

If extravagant unreason in medicine can go any further I am unaware of the fact and incapable of comprehending it. And if from such evidence as is here adduced—while I believe strict justice has been done the monograph—the reader can convince himself that such a disease as "cerebral hyperæmia" in the sense here referred to has any existence outside Dr. Hammond's imagination, his discernment and reasoning faculties are of an order to which I am a stranger.

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